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MacFadden

THE

PARALLEL ROADS OF GLENROY:

THEIR ORIGIN

AND RELATION TO THE GLACIAL PERIOD

AND

THE DELUGE.

BY

JAMES MACFADZEAN.

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The accompanying Map of Lochaber is a copy, by Photo-Lithography, from the Ordnance Survey Maps of the District; and the space that is tinted blue is intended to show the position of the sea when at the level of the highest Glen Roy Terrace.

With respect to my suggestions on the Bible account of the Deluge, I ought to explain that I do not know Hebrew, and that my remarks on the meaning of some of the words in the original, are simply the result of groping my way with the aid of Dr Young's valuable Analytical Concordance.

PREFACE.

THE following paper on the origin of the Parallel Roads of Lochaber, was sent to the Meeting of the British Association, held in Glasgow in 1876. It was set down to be read on the 7th September, but, on the same day, the Duke of Argyll addressed the Section on the physical structure of the Highlands, and as his Grace's address deservedly attracted much attention, there was not sufficient time left to read all the other papers, and my one, being unusually long, was doomed to be "held as read." True, the abstract was ultimately submitted to the Section, but as it had been written before the paper itself, and written in a hurry, it was both meagre and incomplete, and hence it will be readily understood how my hypothesis received little attention.

In these unsatisfactory circumstances my paper was laid on the shelf, where it would probably have remained had not the appearance of Professor Prestwich's Monograph revived my interest in the question. This distinguished geologist adopts the glacial-lake view as affording the most satisfactory solution of the problem, but suggests a somewhat different interpretation of the build of the barriers and the formation of the "roads." According to his view the hills were "covered with a coating, several feet thick of local angular fragments mixed with sand and clay, the product of the great ice sheet." But to explain the *modus operandi*, I had better cite the account given of the supposed formation of the middle "road." On the bursting of the ice barrier in Glen Glaster the glacial lake necessarily fell to the level of the col at the head of that glen.

"As the lake fell rapidly," says the Professor, "and exposed this detrital bank, the saturated mass of debris would part with its water, which, as it drained away, must have caused a loss in volume that led to the displacement of its diverse component parts, and thereby set the whole semi-liquid mass in motion, and caused it to slip or slide down after the falling waters of the lake, with a velocity in proportion to the angle of slope and momentum of the mass. The slide would continue so long as the escape of the lake waters continued, or until their level fell to the level of the Glen Glaster col. As these waters came gradually to rest, their inertia, opposed to the momentum of the detrital mass, combined with the absence of any fresh exposure, and the cessation of the original cause of movement, more or less suddenly arrested the sliding mass, deflecting it in a direction more horizontal and outwards from the face of the hill, and thereby causing it to form a shelf parallel with the water line." . . . . "As the lake again fell to the next level, the slope below the water level would undergo further modification, as, in consequence of the detrital bank being then again set in motion, another slide would commence at the water-level, and carry down a portion of the detritus on this slope to a lower level, either to form another shelf if the waters received another check or *en masse* to the bottom of the valley if no check occurred, and the lake was drained."

Doubtless this modification of the glacial theory saves it from two or three serious objections. First, it is in harmony with the fact that the "roads" resemble stairs or sections of parallel layers applied in succession to the face of the hill; secondly, it saves the theory from the improbable supposition that all the three ice barriers gave way at equal intervals of time; and, thirdly, unlike the lake-beach view, the necessities of the theory do not require us to suppose that the alleged lake stood for a very long period at the level of each of the three cols, and taking into account the uncertain nature of the barriers under an amelioration of climate this last point is a considerable gain to the glacialists. But, granting all this, the Professor's modification does not, as appears to me, account for (1) the fact that each "road" is throughout its whole course very equal in size, (2) it does not account fact that all the three "roads" are very equal to one another, and

(3) it does not account for the fact that where they are narrower they are also steeper than usual. Many persons will also feel disposed to enter a caveat against the assumption as to the land having just been relieved from its covering of ice and snow. That the higher lands of Scotland were, in early tertiary times, above the level of perpetual snow will not be disputed, and in obedience to well-known laws this doubtless gave rise to the formation of glaciers which pushed themselves down the larger valleys probably to the sea. Neither will it be denied that our shores were each winter surrounded by coast ice, for this is proved by the indurated character of the "Till." In many places this formation completely covers the lower hills, and contains the most miscellaneous assemblage of boulders—miscellaneous both as regards extremes of size and extremes in the degree to which they are rounded—and so hard and tenacious is it that the farmer requires to attack it with the pickaxe instead of the spade. Indeed, when the puddled nature of the formation is taken into account with the character of the embedded boulders, it is difficult to suppose any other possible origin than that suggested by the late Mr Trimmer, who attributed the Till to "the action of the shore ice on sinking land." The fact that the hills up to a certain height are rounded probably proves no more than that during a period when atmospheric influences were peculiarly active on the lower half, the upper half of the hills was protected by a coating of perpetual snow. At any rate, the weathering or rounding process is still going on before our eyes. Again, before anything can be made of the striæ on the rocks, the striation, in which the parallel groovings are in a direction independent of the courses of the valleys, must be first separated from that produced by local glaciers and the scratches of floating ice during the submergence of the land. From such considerations, and their number might be multiplied, I do not think that the question as to whether an ice sheet covered all the land is by any means settled in the affirmative. But, waiving the question of an ice sheet, there are, as appears to me, more serious objections to Professor Prestwich's explanation. On the supposition that the detritus on the hillsides was of the loose material which he describes,

doubtless the removal of the supporting pressure of the waters of the alleged lake would communicate to it a tendency to slide down; but under the circumstances, all the uniformity of level would be along the former shore line, because in the case of matter extending for miles along both sides of the valley, it is manifest that it must have been subjected to a great variety of conditions. Here, a favourable slope accelerated the descent, whilst there, from inequalities of the surface, friction retarded it; at one point a greater depth of matter slid down than at another; and, further, we cannot suppose that along so many miles of hillside the material continued throughout of equal tenacity, and with the same proportion of stones, and any difference of this kind must have produced a corresponding difference in the tendency to slide. Now the combined effect of all these causes would give the greatest variety, both as regards size and force, to the sliding masses, and although it is quite true that the pressure of the water must have *tended to arrest* a body sliding down in the manner suggested, yet it seems incredible that a landslip, or rather an endless variety of landslips, could all have come to rest along a line so evenly marked off upon the whole as the "roads" undoubtedly are. At any rate, on the west coast of Kintyre we have a great variety of landslips, both old and new, and instead of exhibiting a horizontal tendency, they are interesting for their very diversity. But if the supposed landslips occurred under such a variety of conditions as I have suggested, the resistance of the water could not have produced equal effects on all the sliding masses. We see a boulder of some tons in weight, and beside it a mass of blown sand. The boulder and each particle of sand are of the same specific gravity, and yet with the boulder we associate the immobility of ages, whilst we expect the grains of sand to be the sport of every gust of wind. The *rationale* of this is, of course, that the wind only acts on the surfaces of the two bodies; but the surfaces are to each other as the squares of the diameters, whereas the *vis inertiæ* is as the cube of the diameters. Suppose, for instance, o masses of the detritus—the one containing say 125 cubic feet, and ther 1000—to have slidden down in the manner suggested, the respective surfaces must have been to each other in the relation of 1 : 4, whilst, on the other hand, the *vis inertiæ* were as 1 : 8; and hence, all other things being equal, the effect of the resistance of the water on the momentum of the smaller sliding mass must have been just double that on the greater one.

On the hypothesis we are discussing, the glacial lake, in order to have formed the highest "road," must have at one time stood at an elevation considerably above the level of the Glen Gluoy terrace, and this not only requires us to raise the ice barriers all round, but requires a new one to block up the col at the head of Glen Roy; and, as I have pointed out in the following paper, there is no known fact to justify us in supposing that ice is capable of being piled to any such height, all the while remaining impermeable to water. This reference to a higher level of the lake suggests another difficulty. In the cases of the middle and lowest "roads," the matter going to form them may be said to have slid down through the same space, and owing to this fact, a certain degree of uniformity would be maintained in the velocity of the sliding masses; but as regards the third, or highest road, the circumstances require us to assume that the source of the detritus was, not like the other cases, restricted to a well-defined zone, and this must have had a very disturbing effect on the momentum of the descending land-slips. Again, according to the Professor's theory, we ought to expect to find, wherever the slope admits of it, a lower terrace still, not parallel, however, to the other terraces, but parallel to the mean fall of the bed of the Roy. Because, if the detritus slid down a step every time the level of the lake fell from one col to another, clearly it ought to have taken a final slide when the lake disappeared from the valley, the only difference being that the bottom of the decline did, in the one case, what the resistance of the water did in the other.

It will perhaps remove a difficulty for me to explain here that, for the sake of convenience, I have in my paper assumed that it was high-water on the one side of the watershed when it was low-water on the other; but, of course, I do not pretend that there was any exact synchronism between the two events. Again, from the want of information with respect to the height and contour of the mountains bounding the Caledonian Canal, I have not ventured an opinion as to whether the meeting of the tides, which must have taken place in that great trough, produced any, or what influence, in the basin of Lochaber. This much, however, may be safely assumed, that the opposite tides had two meeting places, but, in a sense, no ebb. Because, before the tide from the west could have reached Inverness, it must have been met by the flood from the east, and, in this way, the ebb of the tide from one direction, was necesssrily converted into the flood of that from the other. Hence there must have been a to-and-fro motion along the great glen, with two points of meeting. But currents cannot directly impinge on each other without raising the height of the tide at and near the spot, and as the velocity of a current depends upon the depth and breadth of the channel, it is manifest that the two points at which the tides met, must have gradually receded from each other with the deepening and broadening of the current. Now, if we suppose one of the two meeting places, with its exaggerated high-water line, to have passed the mouth of one of the tributary glens, it is clear that, on the one hand, its first approach would be felt as an apparent acceleration of the secular rise of the sea at the spot, whilst on the other, its retreat would be felt as a halt in the upward movement, and this halt was probably the cause of those traces of terraces observed in some of the lateral valleys.

In my paper I have referred to the great engulfment in the Atlantic; but my brief remarks on the subject do great injustice to my argument, for I feel that there is proof to show that the engulfment in question was only a part of convulsions that extended round the world. The upheaval of such mountain ranges as the Pyrenees, the Apennines, the Carpathians, the Caucasus, and the greater portion of the Alps and the Himalayas, belongs to this date. In fixing the date of a dislocation, or a lava-flow, there are of course serious difficulties in the way: first, what is admittedly newest tertiary rock is of very limited area; and, secondly, the age of much of the tertiary formation is undetermined. It is self-evident that any trap rocks immediately underlying the drift may have been ejected at the close of the era, but we would not be justified in assuming that such was the case. Hence, in selecting the following instances of disruption in the crust, I have confined myself to cases in which formations newer than the alleged Eocene have been tilted up, and now form a floor to the drift or later pleistocene deposits. This, of course, does not fix the date of the disturbance absolutely, but considering the large proportion of the surface over which the drift forms a roof to the secondary and Palæozoic rocks, the balance of errors will obviously be against what I am contending for.

Beginning in the far west, we find, in California, abundant proofs of enormous disturbances at the close of the era; the Table Mountains were upheaved at this time, and so completely was the drainage of the country changed, that the gold digger finds his "pay-dirt" in an old river system quite different from the existing one, and following these obsolete channels he has often to mine his way under mountains of lava and other eruptive matter.

Passing to the Atlantic side of the American continent, the Antilles afford ample evidence of a great disturbance. In Jamaica all the strata up to, and including the White Limestone (Pliocene) have been tilted up by a nucleus of igneous rocks; and evidences of similar disturbances have been found in both San Domingo and Antigua, and what is of more consequence still, the prevailing dip of all the principal islands of the group, points to a common centre to which the islands themselves form a sort of off-and-on arc of a circle, and the obvious inference is that a great engulfment took place over the area now occupied by the Caribbean Sea.

The great dislocations in the Isle of Wight and neighbouring regions, show that the disturbances, consequent on the engulfment in the Atlantic, extended to, and embraced the South of England, and this being the case, the importance of the following conclusions by the late Sir Roderick Murchison cannot be over-estimated. The following extracts are from a paper on the flint drift in the South of England :—

"That the mass of the drift accumulated in Pease Marsh and the valley of the Tillingbourne proceeded from the nearest great fracture in the chalk and greens admits, I think, of little doubt. Thus the clinkers that contribute one-half of the detrital matter have been washed in from the surrounding hills of Lower Greens," and further on in the same memoir we are told : "I believe then that the detritus and mud around Pease Marsh was chiefly the result of disturbances of the adjacent strata, *whilst broad and torrential bodies of water were moved along under the escarpment of the North Downs, and translated the debris into natural depressions.*"

Time and space alike prevent me from giving proofs, but I may state that I have in my possession authorities that go to prove that similar upheavals took place in France, Spain, Switzerland, Italy, Sicily, Germany, Austria, Turkey, and Hungary in Europe. With respect, however, to the eruptive rocks of Hungary and Transylvania, there are some points of peculiar interest. Baron Richthofen, speaking of the Carpathians, tells us that the trachyte "has exclusively broken out in masses, following extensive and precisely determined fissures, and towering into large mountain ranges." . . . . The eruption of greenstones—trachytes is a continental one. Subsequently to them the level of the land was lowered, "*so that the grey trachytes were enveloped at the very moment of their breaking out by layers of tuffs partly alternating with them.*"

With the above, consider the following extracts from a memoir by Captain Spratt:—

"The foot of the Carpathian mountains between Fokschan and Rimmi is composed of vast gravel-beds rising several hundred feet above the Wallachian plateau of which it forms the abrupt boundary."

"These gravelly beds seem to have been deposited under great disturbance, and to have been of late origin. They have been derived apparently from the rocks of the Carpathians during some great upheaval accompanied by some *great water-action operating powerfully and extensively.*"

In the Mediterranean Islands the proofs of upheaval are abundant, and Lyell concluded that the oldest eruption in the volcanic regions of Naples *took place precisely at the close of the Newer Pliocene* period; and the same distinguished author informs us that Newer Pliocene marine beds constitute the foundation of Mount Etna. These facts are important in themselves, but more so, on account of the fact that Murchison has shown that the convulsions in Italy cannot be separated from those of Austria and Switzerland.

Professor Foetterle of Vienna, after describing how the older Tertiaries in Asia Minor are remarkable for their great extent, proceeds to cite facts which go to prove that the neighbourhood of the Hellespont has also been the scene of great disturbances in the crust of the earth:—

"The younger Tertiaries running along the south coast of the Gulf of Corinth, and spreading inland to a height of several hundred feet, are solid calcareous conglomerates overlaid by sandstones, marls, and sands, on which are several distinct terraces of horizontal diluvial beds. These Tertiaries rest on limestone of older date, and have been *affected by the upheavals which the limestones have suffered*, sloping steeply to the north."

But any one who doubts a general disruption of the crust of the earth at the time under consideration, ought to read a memoir in the Geographical Journal, Vol. XI., on the Turko-Persian Frontier range, by William K. Loftus, Esq. His survey extended from the head of the Persian Gulf to Mount Ararat, 600 geographical miles; and throughout the whole distance it is clear, from the capital sections that accompany the paper, that all the strata older than the drift have been upheaved, disrupted, and folded in every imaginable form, and in some instances great masses of drift-gravel are *heaped up* "*on the upturned edges of the tertiary strata.*"

"Similar masses of gravel are met with in the plain of Hershel, at the point where the river Shirwan emerges from the gorge of Semiram. They repose horizontally on the edges of the gypsiferous rocks and abut against the side of the Bámú-limestone range. The components of this gravel, wherever met with, are essentially the same, and prove beyond a doubt that the deposit is *everywhere due to the same cause* whatever that may have been." . . . Throughout the whole of this vast accumulation the various pebbles are thrown together without the slightest appearance of order or stratification.

This range is also remarkable for transverse clefts, locally called "Tangs." Each of the rivers, instead of flowing along the troughs that separate the parallel ridges, "takes a direction at right-angles to its former course, and passes directly through the limestone range by means of a 'Tang,' which appears to be formed expressly for the purpose. On reaching the next trough it follows its original S.E. course, and again passes through the next chain in a similar manner." . . . The "Tangs" are not situated at the lowest or narrowest portion of the range, but most frequently divide it at its highest point. After showing that it is out of the question to suppose that the rivers themselves have been in the least degree instrumental in cutting these clefts, Mr Loftus draws the following important conclusion:—

"That 'Tangs' are due to the tension of the cooling mass at right angles to the axis of the chains in which they occur, and have been formed during the cooling of the crystalline mass is, I think, self-evident, the entering and re-entering angles on either side of a Tang exactly corresponding with each other."

Now, when we consider, first, that the upheaval and disturbance along this great range have affected all the tertiary deposits below the drift; secondly, that from the homogeneous nature of the masses of drift material it is clearly in some way connected with the disruption of the crust; thirdly, that the transverse clefts, due to the contraction in cooling, afford proof that the upheaval was sudden; and fourthly, that the range was competently surveyed for a distance of 690 miles—I say when we consider such facts, is it possible to overvalue the evidence which they afford of a wide-spread disruption of the crust at the time in question? But after all, was this Frontier-range more than a mere spur from the much greater convulsion that appears to have extended from the Isle of Wight to China? Speaking of the Lower Caucasus with its lofty summits 12,000 feet above the sea, Professor Abich tells us, that, "along a line which may be said to represent the principal portion of this mountain chain, it is traversed by a succession of table lands connected with one another, having a considerable though not always the same mean elevation above the sea. The rocks of which they consist have been brought to the surface in *an igneous or molten state*. . . . The geological age when these remarkable effects took place is, relatively speaking, very modern; its last appearances are clearly visible in the early dawn of the history of the human race."

According to Baron Richthofen, the eruptive region extends from Persia over Asia Minor and Hungary to the Siebengebirge and the Eifel on the Rhine, and Captain Strachey tells us as regards Central Asia, "the elevated region is known to extend through nearly 30 degrees of longitude, from the sources of the Oxus to those of the Hoang-Ho, he Yellow River of China." Descending to the plain of Hindostan, we know, on the authority of Major Vicary, that, in the Upper Punjaub and Peshawer there is a Pliocene formation which has a dip of 85° to 90°. Again, the Salt Ranges in the same province have, according to Dr Fleming, been upheaved since the deposition of the soft sandstone containing the mammalian bones, and the same remark holds good with respect to the Murree Ranges, 7000 feet above the level of the sea. Further, the Siwalik Hills at the foot of the Himalayas have been thrown up since the deposition of the tertiary strata, containing the famous collection of animal remains, and it is worthy of note that this assemblage of mammals has a significance far beyond the evidence which they afford of a local disturbance at the close of the era, for, as observed by Lyell, "The giraffe, camel, and a large ostrich may be cited as proofs that there were formerly extensive plains where now a steep chain of hills, with deep ravines, runs for many hundred miles east and west."

The late Rev. Mr. Hislop, who examined extensive districts round Nagpore, came to the conclusion that there was but one great outpouring of basalt in Central India, and that it occurred since the deposition of the tertiary strata. But one of the strongest proofs of a general outpouring of igneous rocks in British India remains to be noticed. On the re-establishment of the equilibrium after the convulsions and great waves of translation described in my paper, the sea in the N. Atlantic basin came to rest, perhaps from 700 to 1000 feet below the existing level, but in India it came to rest 500 feet above the existing level, and the consequence is that, along the coast below this level, all the drift gravels have been modified by the action of the breakers, and cemented together by clay and oxide of iron into a formation now known as the coast-laterite. Now, it can hardly be necessary to remark that such a large supply of oxide of iron presupposes an enormous decomposition of trap-rocks. In short, every fact that I have been able to glean, from very limited sources I admit, goes to prove that the principal mountain ranges of Asia were upheaved at the close of the Tertiary era proper, and in order to show that the increment thereby made to the mass of that part of the world was not by any means a myth, I will now call attention to a few of the more palpable facts.

We have seen that the Himalayas, between the points where the range is cut by the Indus and the Brahmapootra respectively, have a length of 1500 miles, and the mean breadth may be estimated at 130 miles, and taking three miles as the average elevation of the range we have an upheaved mass of 585,000 cubic miles; and this is far short of the truth, for, according to Humboldt, the Himalayan range extends through China to the Volcanic island of Formosa, and this eastern extension is equal to at least another 1500 miles. But the western extension of the Himalayas is even more important than the eastern; the Hindoo Koosh, rivalling in magnitude the Himalaya itself, extends through 10° of longitude. After the Hindoo Koosh, the westerly continuation of the range is represented in order by the Ghor mountains, and the Kaitu mountains, and then come several parallel ridges stretching from the neighbourhood of Herat to the Kutu Jaghatai range. The Shah mountains come next, and they are followed by the great chain known as the Elburz, and it runs parallel to the Caspian Sea till it merges in the great Caucasian range, which of itself, has a magnitude of about 300,000 cubic miles. Of course, it is not pretended that there is proof that the whole of this line, extending from the Black Sea to the Island of Formosa, was upheaved at the close of the Tertiary era; but when we find evidence that, at that time, the Himalayas with a length of 1500, and the Caucasus with a length of 600, miles were upheaved, the former range occupying a central position in the line of disturbance, and the latter its western extremity—having proof of this, I think, in the present state of our knowledge there is a reasonable probability that along the whole line of disturbance the upheaval was synchronous.

I have already adverted to the Turco-Persian frontier range, which, beginning in Armenia, passes through Kurdistan and forms a sort of an eastern wall to the great plain of the Tigris. At about 45° north the range trends more to the south-east, and then stretches through the whole of the south-west of Persia and Beloochistan till it reaches the river Indus, and here, at its eastern extremity it is met by the Suliman Mountains, which again may be considered mere off-shoots from the Himalayas. With respect to the Suliman Mountains I have no direct information as to the date of upheaval, but the fact that the Salt range—a lateral spur—was upheaved at the close of the Tertiary era, clearly raises the presumption that the principal range was upheaved at the same time. As justly remarked by Mrs. Somerville, mountains which bound table lands have no existence independent of them, being merely their mountainous faces, and hence when we find that there is a strong presumption that all the mountain ranges that bound the great plateau of Iran were upheaved at a certain crisis in the earth's history, may we not go a step further and conclude that the plateau itself was upheaved at the same time, and if I may be permitted to take this view, its effect upon the equilibrium becomes apparent. We have seen that the effect of the engulfment in the Atlantic must have been to remove the equator farther south on the meridian of Greenwich, and the effect of such enormous masses added to the continent of Asia must have been to draw the equator farther north in the Pacific. Because it must be borne in mind that the axis is fixed by the mean momentum, and the mean momentum again, is the mass multiplied by the velocity.

Did other considerations permit, I might go on adding to the proofs of a universal convulsion at the time in question; but I have made this introduction too long already, and I must close with a few references to the S. Pacific. In Java the *whole tertiary* series has been disturbed, and speaking of Otago, New Zealand, Dr. Hector observes that "nearly all our basaltic rocks overlie the above mentioned fossiliferous limestone (Newer Tertiary), and were deposited to what is now a height of 1800 feet *as submarine lavas* and tufaceous beds. The period of this volcanic energy was one of upheaval." In the province of Auckland we have proof of the same thing, and according to Mr. Heaphy, Provincial Surveyor, some of the eruptions *were subaqueous*, and took place "at the *upheaval of the tertiary beds.*"

Passing to Australia, there are many thousands of square miles of trappean and volcanic rocks, and, according to Mr. Daintree, "the more northern volcanic areas, those shown on the map north of latitude 21°, are probably contemporaneous with the upper volcanic series of the Victorian geologists, so extensively developed in the western districts of that colony. These have issued from well defined craters still in existence, and are probably of *Pliocene Tertiary* age."

Speaking of the colony of Victoria, Mr. R. B. Smyth observes that, "the area occupied by recent basalts is at a low computation three thousand five hundred square miles." . . . . From a careful inspection of a large extent of country covered by basaltic rocks, and from information collected from various sources, I incline to the opinion:—

1st. That the newer basalt was erupted at a period when considerable areas, both north and south of the main coast-range, were submerged, but the appearance of much of the basalt shows that it must have cooled rapidly and not under very great pressure.

2nd. These eruptions do not appear to have disturbed the "miocene" tertiary beds which are generally found "nearly horizontal" [the basalts overlie the Tertiary beds.]

3rd. That from the occurrence near many extinct volcanoes of porous lava, vesicular basalt, and obsidian, associated with fragments of white pumice, it is probable that subsequently to the deposition of the latest tertiary drift (Pleistocene) which overlies the newer basalt, some of these volcanoes were active, but were not erupting lava in such large streams as at former periods."

This interposition of the drift between the beds of lava is just what might have been antecedently expected, because, on my view, the convulsions began when Australia was at least 1000 feet more under the sea, and it is highly probable that the outpouring of eruptive matter continued for some time after that continent had risen above its present elevation. This relation which exists between the trap and the drift is interesting from another point of view, for, according to the Rev. Mr. Clark, precisely the same dovetailing exists between the two formations in California. Nay more, there is not only this correspondence, but there are also in each country two sets of river courses—the older one being filled up with the newer drift, and the importance of this agreement will be seen when it is remembered that in Australia and California the search for gold has not only led to a far more careful study of the superficial deposits, but the extensive mining operations have also afforded better opportunities for observation than in other parts of the world.

Finally, I beg to append the following summary of the bibliography of the Parallel Roads, for which I am indebted to William Jolly, Esq., F.R.S.E., H.M. Inspector of Schools, Inverness. ("Nature" Vol. xxii., page 69):

THEORIES.		*WRITERS.*		*WORKS.*
By Man.—Human.	Fingal.	Old Inhabitants.		
	Hunting Roads of Kings.	Old Inhabitants.		
		Pennant.	1769	Pennant's "Tour." 1771.
	Aqueducts for Irrigation.	Playfair.	1816	Proc. Roy. Soc. Edin. 1816.
By a Flood.—Diluvial.		Sir Geo. Mackenzie.	1848	Edin. Phil. Jour. Jan. 1848.
		Prof. Rogers.	1861	Lect. Roy. Inst. March 1861.

THEORIES.		*WRITERS.*		*WORKS.*
By the Sea.—Marine.		Darwin.	1839	Lond. Phil. Trans. 1839.
		Lyell (visited 1825)	1841	"Elements of Geology."
		Chambers.	1848	"Ancient Sea Margins."
		Rev. R. Boog Watson.	1866	Geol. Soc. Lond. Jour. Feb. 1866.
		Professor Nicol.	1869	Ditto. August 1869.
		Campbell of Islay.	1877	"Parallel Roads of Glenroy" (printed privately).
By a Lake—Lacustrine.	Nature of Dam doubtful.	Macculloch.	1817	Trans. Geol. Soc. London (1st series). Vol. iv.
		Lubbock.	1868	Geog. Soc. Lond. Jour. May 1868.
		Babbage.	1868	Ditto. August 1868.
		Rev. T. Brown.	1876	Proc. Roy. Soc. Edin. Vol. viii. March 1876.
	Dam of *Dèbris.*—Detrital Dam.	Dakyns.	1879	Geol. Mag. Dec. 1879.
		Dick Lauder.	1823	Trans. Roy. Soc. Edin. Vol. ix.
		Milne-Home.	1847	Proc. Roy. Soc. Edin. 1847.
		Do.	1876	Trans. Roy. Soc. Vol. xxvii. 1876.
		Do.	1877	Ditto. Vol. xxviii. Part I. 1877.
	Dam of Ice.—Glacial Dam.	Agassiz (visited 1840).	1842	Geol. Soc. Lond. Jour. Vol. iii. 1842.—Atlantic Monthly. June 1864. (Both by Agassiz.)
		Buckland (visited 1840)	1842	
		Jas. Thompson.	1848	Edin. New Phil. Jour. Vol. xlv.
		Jamieson.	1863	Geol. Soc. Lond. Vol. xix. Jan. 1863.
		Darwin.	1863	In Private Letters, &c.
		Lyell.	1863	Antiquity of Man.
		Archd. Geikie.	1865	Scenery and Geology of Scotland.
		Jolly.	1873	Trans. Geol. Soc. Edin. April 1873.
		James Geikie.	1873	"The Great Ice Age."
		Sir Henry James.	1874	"Parallel Roads of Lochaber," Ord. Sur. Office.
		Tyndall.	1876	Roy. Inst. June 1876. Popular Science Review. Oct. 1876.
		Prestwich	1879	Roy. Soc. Lond.—Nature. May 29, 1879. In abstract Lond. Phil. Trans., 1880, in full.
		Jolly.	1880	Nature. Vol. xxii.

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November, 1882.

THE
PARALLEL ROADS OF GLEN ROY.

SEC. I.—THE CONDITIONS OF THE PROBLEM.

Owing to their striking appearance, and the difficulty of explaining their origin, the Glen Roy "roads" have attracted so many tourists and men of science to the spot that the facts of the problem to be solved are very generally known: still it may be well to cite the following capital summary by Sir John Lubbock:—

1st. The side of the hill is covered with rubbish derived from the weathering of the rocks.

2nd. The roads are not excavations in, nor embankments on, the side of the hills; but in the words of Macculloch resemble stairs or sections of parallel layers applied in succession to the face of the hill.

3rd. The horizontal roads do not appear when the solid rock appears, nor when the slope is exceptionally flat.

4th. Macculloch only saw one case in which there was a superior talus, and never found any trace of an inferior one.

5th. The stones on the road are very little rounded.

6th. The roads are quite continuous except under the circumstances named above, or where a rivulet runs down the side of the glen.

7th. The roads slope towards the valley.

8th. The roads are very equal in size; not only is this the case with each road during its whole course, but it is also true of them as regards one another.

9th. When however they are narrow, they are also steeper than usual.

"The true explanation of the origin of the roads must be consistent with all these conditions."

To the above may be added the now well-known fact that at nearly the same level with each "road" there is a col whose summit-level forms the dividing line between the two water-sheds. Another important circumstance is, that although the "roads" look perfectly horizontal, yet, as shown by the Ordnance Survey, they are not really so, but curved in a manner that will be discussed in the sequel.

SEC. II.—OBJECTIONS TO GLACIAL THEORY.

This explanation, at first proposed by Agassiz, is, at present, alone in favour. According to the distinguished Swiss philosopher, ice-barriers at the mouth of the glen dammed back the water, forming a lake; and the roads are simply the ancient shore-margins at which the water-level remained for a time stationary. And the discovery that there is a correspondence between the levels of the roads and the respective cols has, in the hands of Mr. Jamieson of Aberdeen University, not only given this explanation very plausible support, but may be said to have restored it to life. Notwithstanding, however, all the high authorities that give their assent to this theory, I cannot help thinking that it is untenable, and I will now attempt to give my reasons for this opinion.

Dr Carpenter has ascertained that, where undisturbed by currents, the temperature of the ocean below the zone affected by the heat of summer is at all depths the same; and that this constant temperature, in deep water, *is determined* by the maximum cold, at the surface, in winter. The explanation being that the cold water, owing to its greater gravity, sinks till it reaches a stratum of its own temperature. In the case of a mass of ice, however, the law is just reversed. A glacier has neither a uniform solidity, nor does it move at a uniform rate, and one part may be in motion whilst the other is at rest. Under these conditions a mass of ice becomes full of rents and crevasses, through which streams of water, formed on the surface, fall to the bottom, carrying with them the temperature of summer;

and the heat thus carried down gets beyond the reach of the winter's frost; and, owing to the operation of this law, streams excavate channels for themselves beneath the ice. This is so much the case in Greenland that rivers of this description flow there both in summer and winter, and such rivers must be of some size, for voyagers tell us that seals swim up them under the ice in pursuit of salmon. Now, keeping these circumstances in mind, let us look at the calls which the glacial hypothesis makes upon our credulity. From the position of the "roads" the ice-barrier must have been at least 1000 feet deep, and five miles long; and we are asked to believe that this stupendous mass kept moving forward over an uneven surface in front of the valleys of the Roy, the Spean, and the Gloy; here, rent into a great crevasse, and there, undermined by glacial streams; contracted at one time by the cold of winter, and, at another, expanded by the heat of summer: we are asked, I say, to believe that the great ice-barrier kept moving along subject to all these varying conditions, and all the while presented an impermeable front to a lake seven or eight hundred feet deep. And this is not all. The temperature of the supposed lake being, of course, above the freezing point, its waters necessarily acted as a solvent upon the glacier. To this is to be added that ice being of less specific gravity, a depth of seven or eight hundred feet of water must have exercised a powerful influence in breaking it off in masses in the same manner as bergs are now set adrift in Baffin's Bay. For only one embankment, subject to all these accidents, to remain impervious to the pressure of the pent up waters, would, of itself, be without precedent; but, when it is remembered, that the necessities of the theory require two other glaciers, equally water-tight, it will be seen how incredible the whole thing becomes.

My next objection is, that the glacial theory entirely fails to account for the general uniformity in the breadth of all the three "roads," and the fact that at all places where they are narrower than usual they are also steeper gives to this uniformity very great significance. Because, bearing in mind that the "roads" are built up of the detritus on the hillside, and that they are not equidistant from each other, this relation, which the steepness bears to the narrowness, appears to shut us up to the conclusion that the general correspondence in the breadth has been caused by a general correspondence in the time during which they were respectively formed.

But if we conclude that each of the three roads required the same time for its formation as the other two, then we are face to face with the improbable assumption that the respective ice-barriers disappeared at equal intervals of time.

A third objection is with respect to the removal of the alleged barriers. So far as I am aware all glaciers and land ice, under an amelioration of climate, retire first from the low ground, but, on the glacier hypothesis, it is necessary to take it for granted that the higher glaciers were the first to disappear. Between the level of the river Spean opposite Loch Treig and its level at Spean Bridge there is a difference of 400 feet, and this must have made itself felt on the temperature between the two points. Moreover, owing to the prevailing south-west winds blowing up Loch Linnhe, snow does not fall often or lie long in the basin of Lochaber; and if we may suppose that the prevailing winds were in the same direction at the time under discussion, we have, in the circumstance, another reason why we ought to expect the lower glacier to have retreated first in order of time. True, it may be alleged that the land stood higher then than now, but that only shifts the difficulty without removing it, because such a supposition increases the improbability of the valleys, at a higher level, remaining brimful of water, whilst the lower and larger basin remained packed with ice.

There is a fourth difficulty in connection with the supply of the material of which the "roads" are built up. It will be remembered that they are neither excavations in, nor embankments on, the side of the hill; but, in the words of Macculloch, resemble "stairs or sections of parallel layers applied to the face of the hill;" and manifestly the conditions, which this description implies, cannot be satisfied unless we assume that the material of which the "roads" consist came down from above. But, if we admit this, we must place along side of it the other fact, that the system of ice-barriers, upon which the theory is founded, requires it to be assumed that the uppermost road was first formed, and this brings us in front of the difficulty. The detritus, of which the middle road is formed, either came from above the level of the upper one or it did not. It could not have come from the higher region, as such a supposition would oblige us to believe that the shingle passed the road, already formed, without either filling it up to the natural slope or otherwise obliterating its symmetry. But if it is inadmissible to suppose that the shingle of the inferior road came from above the higher one, the other alternative does not help us over the difficulty. Because, between the highest road and the middle one, there was not sufficient surface to furnish the detritus of the latter; and, in considering this point, we will fail to realise its importance unless we bear in mind that the "roads" are neither an embankment on, nor an excavation in, the face of the hill. Hence, in the case at least of the two lower roads, the onus rests with the glacialists to refer us to the source whence the detritus was derived.

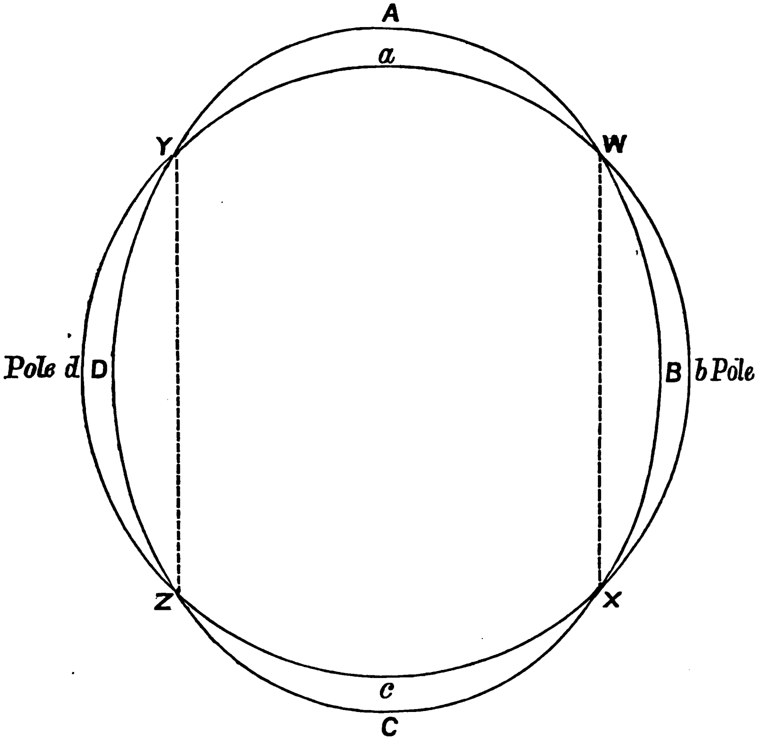
According to the ice-barrier view, the retreat of the Loch Treig glacier permitted the waters to escape by the Glen Glaster col, and this fixed the new level of the loch. But this escape of the waters into Glen Spean creates a serious difficulty. The exigencies of the theory require us to believe that there were no means of exit towards the West, and hence the waters must have flowed through the Makoul Pass into the Spey. In other words, so soon as the barrier opposite the Glen Glaster col burst, the escaped waters must have filled up the Loch Laggan area to the level of the Pass of Makoul. But the glacialists expect us to believe that during the whole time in which the middle "road" was being formed all the space fronting the common mouth of the Roy and Spean valleys remained blocked up with ice to a depth, at least, of 800 feet. And what does this mean? It can only mean that the middle "road" in Glen Roy, and the supposed extension of the lowest one, as seen in Glen Spean, were formed simultaneously, and this leaves unexplained the corresponding extensions outside the mouths of both glens. True, we can suppose a farther retreat of the ice down Glen Spean until it reached a line between Coirecoille on the south side, and the Oahar Burn on the north; but any such supposition only adds to the improbability of the whole thing, because it implies that after the glaciers disappeared from the higher ground of the basins of Lochs Treig and Laggan, they remained over the lower ground during the whole time that the middle and lowest "roads" were being formed, as an impassable barrier, and capable of resisting the pressure of a lake at least 650 feet deep.

The late Professor Nichol has proved the existence of another objection which, of itself, ought to be considered fatal. Manifestly, what is now the river Roy must, on the glacial hypothesis, have at one time or another flowed for long periods through the two upper cols, and the united waters of the Roy and the Spean, for a longer period still, through the Makoul Pass. A river of the size, and in the first instance, with the fall which the circumstances demand, could not have flowed over the cols in question without excavating a channel for itself; but the lamented Professor, just cited, examined all the three outlets in question, and could discover not a trace of river courses. The publication of the maps of the Ordnance Survey ought to have given a quietus to the glacier and every other kind of lake theory, because the levels of the roads, as now ascertained, are altogether inconsistent with the lake-margin view. Further on I will explain that, according to my ideas, the roads were formed by the joint action of the waves and tides during a gradual depression of the land, and I will then have occasion to return to the irregularities in the levels, and hope to show that whilst, on the one hand, the levels are inconsistent with the glacial theory, they are, on the other, just what might have been antecedently expected on the marine theory. I have reminded you that, to satisfy the conditions, the time required to build up any one of the three "roads" must have been about the same as that needed for either of the other two, as without the equality in the times it is impossible to account for the general uniformity in the breadth of all the three. Hence, if they were formed during a depression of the land, it becomes of moment to ascertain the cause of the equableness in the oscillation of level, and to this question I will now address myself.

SEC. III.—FIGURE OF THE EARTH.

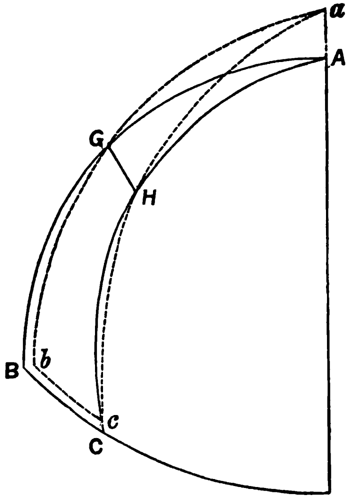
As you well know, the earth is not a perfect sphere, the diameter in the plane of the equator being calculated to be about 26 miles greater than that through the poles. But, although this familiar fact appears simple enough, yet it will mislead us greatly unless we bear in mind that the statement conveys nothing more than that, the surface of the ocean, as measured from the centre of the earth, stands 13 miles higher on the equator than it is supposed to do at the poles. For there is not, by any means, the same difference between the equatorial and polar diameters of the solid sphere; and, as correct views on this point are necessary to the understanding of what follows, I must attempt to place the matter in as clear a light as I can. As it is on all hands admitted that the spheroidal form of the earth is due to its diurnal rotation, I do not see a better way to make my meaning plain than to suppose the rotation to cease. This would cause the waters of the ocean to take the form of a sphere; but, as regards the solid crust, we will assume it to possess sufficient stability to remain in its present shape with all the existing inequalities of surface. A figure will help me to convey my meaning.

FIG. I.



Let *A B C D* (Fig. 1) represent a great circle of the earth in its present form, and *a b c d* the corresponding circle of my hypothetical sphere, both having a common centre. The dotted lines *W X* and *Y Z* are parallels of latitude which for convenience I will for the present assume to be 45°. Now, the question for which I am seeking an answer is:—What depression at *A* is required to raise the points *B* and *D* respectively to *b* and *d*? On first thoughts we run some risk of falling into the error that the effect would be simply a fall of 6½ miles on the equator, against the same rise at each pole. Or, on a more careful consideration, we will probably conclude that the rise, at each pole, would be double the amount of the fall on the equator. Because, by multiplying the conjugate into the square of the transverse diameter, the cube root of the product gives us a fall of 4.4 miles on the equator; but the method is founded on the assumption that the meridian lines are in the form of a symmetrical ellipse: but instead of the earth being a perfect spheroid there are, as appears to me, good grounds for believing that the equatorial protuberance goes nearer the poles than is usually taken for granted. If we take the pole for a vertex, and a degree of the equator for a base, a hemisphere is divisible into 360 spherical triangles all similar and equal; and, therefore, everything as regards curvature and area that applies to one applies equally to all, or to the whole.

FIG. II.



Now, assuming, for the present, that the whole earth is covered with an orb of water, let *A B C* (Fig. II.) be such a triangle, *A* being the pole, *A B* and *A C* respectively two meridian lines, *B C* one degree of longitude on the equator, and *G H* the 45°th parallel: and, again, let *a b c* be in the surface of my hypothetical sphere, *a b* and *c* being, of course, all equidistant from the centre of the earth. A glance tells us that to convert the spheroid* into the sphere, the deepest part to be filled up is the vertex of the triangle *G A H*, whereas the quadrilateral space *B C H G*, now above the mean level, is not only larger in area than *G A H*, but the part where the greatest amount of depression is required, extends along the whole arc *B C*.

In the Edinburgh Ency., under Geography, the area of every zone of one degree, between the equator and the pole, is given, ***that of the whole globe being unity.*** On making a total of all the zones from the equator to the mid lat. we get the sum 353,555, and between the mid lat. and the pole 146,445. These results are, to each other, as five to two nearly; that is, if, by the line of latitude 45° we divide the hemisphere into two zones, five-sevenths of the whole are on the one side, and only two-sevenths on the other. And there is more than area to be taken into account. It is easy to see from figure I. that the earth protrudes above my hypothetical sphere, as a great equatorial belt, and the sphere again above the earth as two great polar zones. The lines in which the belt meets the sphere are, of course, parallels of latitude, which, for the present, I have taken to be 45° N. and 45° S. Well, then, taking these parallels as lines of departure, let us now assume, that, in going north in our latitudes, or south under the Southern Cross, the surface of the ocean gradually becomes lower than my hypothetical sphere, till on the poles it is 6½ miles nearer the centre of the earth; and on the other side of the same parallels it (the surface of the ocean) gradually rises above the sphere, till, on the equator, it stands 6½ miles farther from the centre. A depth of 6½ miles is nearly 11,430 yds., and this number is convertible into a series of 45 terms, each term being 254 yds. less than the preceding one. And taking the areas from the Edinburgh Ency. as before, and beginning at the equator, we can multiply the area of the first zone by the maximum depth of the equatorial protuberance, viz.—11,430 yds., the highest term of the series, and the next zone from 1° to 2° by 11,176 the second term; and so on till we reach the lowest and last term of 254 yds., which is, of course, multiplied into the area of the zone between 44° and 45°. On making a total of all the products we get for the protuberant body of water between the equator and 45°, 2179·658. And, keeping this in mind, let us now calculate the cubic capacity of the space, north of 45°, supposed to be between *the existing surface of the sea and the surface of my hypothetical sphere.* Proceeding, as before, we multiply the area of the zone between 45° and 46° by 254 yds., the lowest term of the series, and that between 46° and 47° by 508 yds., with the same increase for every degree of latitude, making, of course, the last, from 89° to 90°, 11,430 yds. On again summing up the products we get a total of 588·513. This, on the assumption that the medial line is on lat. 45°, means that to convert the globe, here supposed to be covered with an orb of water, into a sphere, the fall on the equator would be, to the rise on the poles, as 588 to 2179, or as 10 to 37 nearly. And putting it in the form of an equation, we have:—

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* The word "spheroid" is used in a loose sense.

$$37x = (13 - x)\ 10.$$
$$x = 2{\cdot}765 \text{ miles.}$$
$$= 14{,}600 \text{ feet.}$$

Thus, were the tropical seas depressed sufficiently to form my hypothetical sphere, the extent of the rise of the land on the equator, would about equal the height of Mount Blanc. Moreover, this is a result that has been obtained on the assumption that my hypothetical sphere cuts the actual surface on the parallels of 45°; and there are good grounds for believing that the protuberant belt extends nearer to the poles.

FIG. IV.

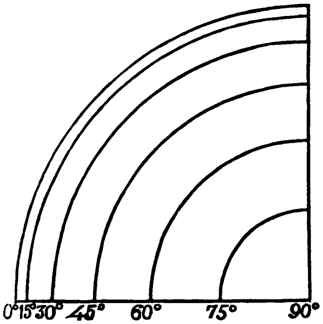
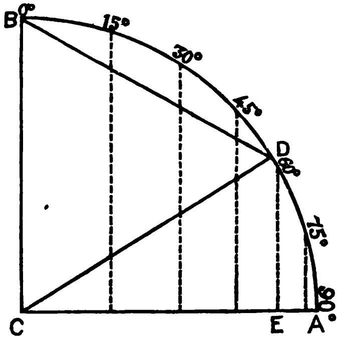


FIG. III.



In figure III., *A B C* is a quadrant of a great circle, *A* being on the pole, *B* on the equator, and *C* at the centre of the earth. The perpendicular dotted lines are the radii of the planes of the latitude at every 15°; and, consequently, the line *BD* is the side of a regular hexagon, and *DE* the half of another side. From this it follows that at 60° lat. the radius of rotation is just half that on the equator. In figure IV. the parallels of latitude corresponding to the radii in figure III. are supposed to be projected on the plane of the equator, and to exhibit to the eye *the relative sizes* of the circles of rotation at every 15°, and it will be observed that *the rate of increase* of the velocity is much greater in high than in low latitudes. Or, to put it in round numbers, the velocity on the equator is 1500 feet per second, and as the velocity varies with the sine of the lat., we have:—

Lat.	Feet. per Second.	Increase of Velocity every 15°.
90°	00	00
75°	388	388
60°	750	362
45°	1060	310
30°	1299	239
15°	1448	149
0°	1500	52

Thus, between the pole and 45°, the velocity increases 1060 feet per second against only an increase of 440 feet in the other half of the quadrant; and this appears to suggest a bulging out in mid latitudes.

When we speak of the centrifugal force we are apt to think loosely of something flying off from the circle of rotation. To use a very familiar illustration, a slinger can whirl round a stone with a velocity of 100 feet per second: and this being so, why are we, moving as we do, at ten times the rate of the stone, not like it flirted into space? The answer is, that, owing to the sharpness of the curve of revolution, the stone is deflected from the direction of the tangential force 90° 0′ 0″ in one-twelfth of a second; whereas an object on the equator is deflected only 0° 0′ 1.25″ ($\frac{1}{2880}$ of a degree) in the same fraction of a second. This means that on the equator the direction of the tangential force differs little from the apparent plane of the earth's surface. Indeed, so little is the difference, that were gravity at any instant to cease to act, and an object to move forward subject only to the centrifugal force, it would, at the end of the first four miles, be only ten feet above the surface; whereas an object similarly set free at lat. 60°, would, in travelling over the *same space*, rise twenty feet above the surface, For this reason, the tendency which *the direction of the centrifugal force* gives to the ocean to fly off from the surface is, on the parallel of 60°, double that on the equator. Hence, *supposing everything else to be equal* this of itself must favour a bulging out in mid latitudes.

In a deduction drawn from what he describes as first-rate pendulum observations, Sir George B. Airy, Astronomer Royal, remarks as follows:—"But it is impossible to avoid remarking that in the high north latitudes the greater number of errors have the sign +, and that about the latitude 45° they have the sign − ; those about the equator being nearly balanced. No alteration in our numbers is sufficient to correct this. If we increased the multiplier of $\sin^2\lambda$ we might make the errors at high latitudes as nearly balanced as those at the equator; but then those *about latitude 45° would be still greater than at present.*" . . . Again, in a note:—"The reader who has taken the trouble to examine our section on Meridian measures, and its results, will judge how uncertain the dimensions of the earth still are, and how certain it is that using any dimensions whatever we must still suppose some disturbing cause to affect the latitudes with errors far greater than the errors of observation." [Figure of the earth.]

Therefore, in addition to the equatorial protuberance there is, as I believe, a protuberant belt in each hemisphere due to the causes adverted to above. And manifestly any bulging out along the parallels of mid latitudes must be taken into account as an important factor in any attempt to calculate the amount of fall on the equator required to convert the earth into a perfect sphere. The series of "first-rate" pendulum observations, discussed by Sir George, indicates that the bulging out is between 41° and 51°. And if I may adopt these parallels, the data must be altered accordingly; instead of my hypothetical sphere cutting the surface on the parallels of 45°, we must substitute 51°. This, of course, increases the protuberant area at the expense of the area to be filled up; and on applying the previous method of calculation to these amended areas, we get for the volume of water to be depressed to the mean distance from the centre of the earth 2425·363, and for the cubic space to be filled up the corresponding figures are 448·016. These results, which are to each other as 541 to 100, give us for the depression on the equator:—

$$\begin{aligned} 541\,x &= (13 - x)\ 100. \\ x &= 2028 \text{ miles.} \\ &= 10{,}707 \text{ feet.} \end{aligned}$$

It ought to be explained that these ratios have been obtained without assuming an actual bulging out between 41° and 51°: all that has been claimed is that the protuberance, from a maximum on the equator, evenly thins out to the respective vanishing lines on the parallels of 51°.

As regards the ratios—viz., 100 : 375 and 100 : 541—used in the foregoing equations, exactly the same results can be obtained by using the rule for finding the solid contents of a wedge. By this method the volume of the protuberant body of the ocean can be got by one operation; but in calculating the space to be filled up we must first bisect it. This forms two wedges, the bisecting plane being *the top of each.* The dimensions of each wedge are easily got, and the sum of the two is the volume of the space to be filled up so as to convert the spheroid into the sphere.

Heretofore I have proceeded on the supposition that the whole earth is covered with an orb of water; but, instead of this being the case, nine-twelfths of the surface between 50° N. and 50° S. is covered with water; and only five-twelfths between these parallels and the poles. And were it not that the transition from the form of the spheroid to that of the sphere would submerge dry land in high latitudes, and at the same time cause the emergence of limited areas in low latitudes—were it not for this, the equation for the fall on the equator would require to be modified as follows:—

$$\begin{aligned} \tfrac{9}{12}\,(541x) &= (13 - x)\ 100 \times \tfrac{5}{12}. \\ &= x1{,}210 \text{ miles.} \\ &= 6{,}338 \text{ feet.} \end{aligned}$$

Thus the fall of the first 100 feet on the equator would be equal to a rise of 1179 feet on each pole; but manifestly this ratio would hold good for only the early stages of the change; because, as the transposition of the waters proceeded, the submergence of land on the one side, and its emergence on the other, would make the data more and more fallacious, till, in the end, the balance would be on the other side of the scale.

All that is contended for under this head is that a proportionably *greater volume* of dry land (and ice set adrift) would be submerged over the two polar zones than could emerge between the. parallels at 51°. I do not pretend to estimate the amount of this difference, but the suggestion may be risked that it would at any rate reduce the previous result to under two miles. And if, to convert *our existing seas* into a perfect sphere, requires only a depression on the equator of two miles, where, I must ask, is that great equatorial protuberance that is said to place the permanence of the earth's axis beyond the range of rational doubt? Water, unless in tanks, is useless, as we all know, as ballast; and, for the same reason, the oceanic protuberance neither fixes nor adds to the stability of the axis. This, however, needs a word of explanation. Although the ocean cannot, of itself, influence the axis, yet it may greatly exaggerate an oscillation due to another cause. A shifting of *the solid mass* displaces the equator, we shall suppose, five degrees nearer Greenwich, with the reciprocal southing in the Pacific. In such a case the protuberant body of water moving with the equator, north on our meridian and south at the antipodes, would greatly add to the amount of the displacement. In short, the disturbing cause would go on increasing till counterpoised by the emergence of land over the S. Atlantic and N. Pacific quarters of the earth.

But to return to the effect of a fall of two miles on the equator. Unfortunately great uncertainty attends deep-sea soundings, and much of the ocean is still a *terra incognita.* Mrs. Somerville has, however, hazarded a mean depth of four miles, but as the depth shoals towards each pole, this is probably too high for the whole ocean. Between the tropics, however, recent soundings indicate a depth of from three to five miles; and, if we take four as a mean, a fall of two on the equator would not, in the Torrid Zone, seriously disturb the present distribution of land and water. For, it is to be remembered, that a fall of two miles on the equator becomes only one on the tropics, and vanishes altogether at mid latitudes. Hence, were the waters of the ocean converted into my hypothetical sphere, the existing dry land areas and reliefs would be affected in something like the following manner:—Although nothing is known of the height either of the Antarctic continent or of a large area in the North Polar basin; yet the probability is that my hypothetical sphere would convert the Frigid Zones into two, all but unbroken sheets of water. As regards the two Temperate Zones, the one half of each would be an area of elevation, and the other of subsidence. In the one, shallow seas would become dry land, and in the other, low-lying land would become shallow seas. Again, with respect to the Torrid Zone, the central regions of equatorial Africa would be elevated to nearly the present height of the plateau of Tibet; the most of the islands of the Indian Archipelago united to the continent; and the Andes of Ecuador probably recognised as the highest mountains in the world. On the three great oceans that now cross the equator, the effect would be even less. Instead of four miles, the mean depth, it is true, would be only two; but, as regards area, the change would be marked by the cession of a mere fringe of land to the great continents. Taking this fringe as equal in area to the existing dry land in the zone, we get the following proportions, viz.:—One-fourth of the surface covered with land of a mean height of 8420 feet, and three-fourths by water of a mean depth of two miles. Or to put it otherwise, were all the dry land levelled down and spread over the bed of the ocean it would leave *the Torrid Zone of my hypothetical sphere an unbroken sheet of water of about one mile in depth.*

SEC. IV.—THE AXIS FIXED BY THE MEAN MOMENTUM.

The plane of the equator necessarily lies in those points of the sphere where the combined effect of the mass and the velocity produces the maximum momentum; and it is hardly necessary to remark that the plane of the maximum momentum again fixes the axis of rotation. The momentum of a body in motion is found by multiplying its weight by its velocity. If we take every separate cubic mile of the mass of the northern hemisphere, and multiply it by its velocity—the velocity, of course, varying with the distance from the axis—and again if we perform a similar operation with respect to the southern hemisphere, the respective results when summed up ought to be practically the same. In other words, the momentum of the one hemisphere necessarily forms an exact equipoise to that of the other. Hence, it follows that the equilibrium may be destroyed by any one of the four following causes:—(1) An addition of matter to one hemisphere without a corresponding equipoise to the other; (2) a removal of matter will, by reducing the momentum, cause an oscillation in the opposite direction; (3) an elevation of land will, by adding to the mass at that particular spot, as well as to its distance from the axis, increase the momentum; and (4) a depression of land will produce corresponding effects, but on the opposite side of the scale to that due to an elevation.

Any one who doubts that the axis of rotation depends in this way upon the distribution of the mass, may satisfy himself by a very simple and easily performed experiment, and in such a case an experiment is worth a thousand alleged mathematical demonstrations. Take a wooden ball—the nearer a perfect sphere the better—and mark on it with paint, or in any other way so as readily to catch the eye, a line representing the equator. When such a ball is rolled along an even surface it maintains throughout its course the same axis of rotation with which it left the hand. But vary the experiment; countersink, at equidistant intervals on the imaginary equator, three pellets of lead, the lead being inserted in such a way as not to affect the evenness of the surface of the sphere. On repeating the experiment under these altered conditions, it will now be found that, whatever may be its axis of rotation on leaving the hand, the ball will at once readjust itself till the axis is at right angles to the plane marked off as an imaginary equator. The effects may be illustrated by further modifying the experiment. If we remove, say one of the pellets of lead 45° towards either pole, we will find that the marked off circle has ceased to be coincident with the actual equator, which now takes up a position corresponding to the mean momentum produced by the three pellets of lead. In experimenting with the ball, it will be found that its mass exercises no influence in determining the axis, the plane of the maximum momentum being determined solely by the difference of the specific gravity between the wood removed and that of the lead by which it was replaced; and, it need hardly be added, that, owing to the form of a sphere and plane respectively, the ball, in these experiments, only comes in contact with the surface in a point, and therefore it is perfectly free to adjust its axis of rotation in obedience to the momentum; and, similarly, the earth suspended, as it is, in space is equally free, or, if possible, more so, being altogether unimpeded by friction arising from inequalities of surface.

SEC. V.—POSITION OF THE OCEAN AT BEGINNING OF OUR ERA

If we take the quarter of the sphere lying north of the equator, between 80° W. and 100° E. long., there is not, so far as I am aware, any early Tertiary formation above the level of the sea.* With respect to the Eocene formation of the London and Paris basins, it is known that both have been areas of disturbance, and that the sediment was originally deposited to a depth of some four or five hundred feet, the estuaries all the time remaining shallow. In contrast to this, all the Tertiary deposits (that is, when we leave out the Pleistocene) in South America and South Africa, are Early Tertiary, in which all the shells are extinct. These early Tertiaries skirt both sides of S. America, and Darwin examined them for 1800 miles in one direction without discovering a trace of disturbance. Again, what may be called the New Zealand quarter of the sphere, is exactly in the same position as the North Atlantic. No early Tertiary, corresponding to Lyell's Eocene, is found in either Australia or New Zealand, but the nearer the coast line the geologist goes he finds older Tertiary than farther inland. In the North Pacific the circumstances are again reversed. On the coast of China there is a margin of early Tertiary rocks, but between these and the Chinese Loess there is a complete blank. Again, there is early Tertiary in the Philippines, and along the north-west coast of America, but no later Tertiary except some disputed formations in California. But this was a region where the rocks were very much disturbed *at the close of the Tertiary era.* I have said that the meridian line of 80° was about the western boundary of what I would venture to call the then North Atlantic elevated quarter of the sphere, and as this line skirts the western shore of South America, crosses the Isthmus of Panama and Caribbean Sea, leaving the West Indies on the east, and Florida on the west, and forms a common meridian to Charlestown in South Carolina, and Toronto in Canada, the question becomes one of great interest as to whether, in early Tertiary times, the land in northern latitudes, to the west of this line, was below its present level, and that to the east of it above its present level.

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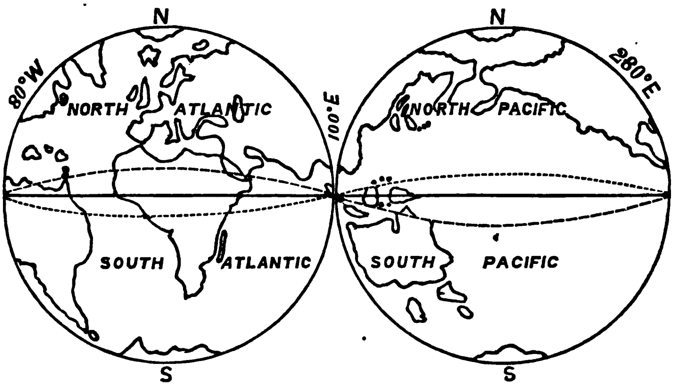
* See figure V , page 19.

From the circumstance that American geologists, whilst admitting that the Tertiary strata on that continent cannot be satisfactorily divided into epochs, have used Lyell's nomenclature, and in their manifest desire to have every formation classed under one or other of the three heads, it is sometimes very difficult to conclude whether a surface formation represents the close of the period or not. For instance, in North and South Carolina there are beds which occupy depressions in the Cretaceous and older Tertiary formations, and are known as the Sumter epoch. These beds have been classed as Pliocene, but the following quotation shows what extreme uncertainty hangs over the question: "Unless," says Dana, "these beds are the equivalents of the Virginian Miocene, they are not represented as far as now known in Virginia or further north. No beds of this epoch have been reported from the Gulf States." Speaking of the Tertiary deposits which cover the low border land between the Alleghany Mountains and the Atlantic, Lyell thus classes them: "It (the Tertiary formation) consists in the South as in Georgia, Alabama, and South Carolina, almost exclusively of Eocene deposits, but in North Carolina, Maryland, Virginia, and Delaware, more modern strata predominate, which, after examining them in 1842, I supposed to be of the age of the English Crag and Faluns of Tourraine."

Now, what I wish to call attention to here is that the older deposits are to the west of 80°, whereas the newer ones are to the east of this meridian, and going further west the matter becomes a certainty, for the American geologists have abundantly proved that the Gulf States formed an area of elevation, large additions having been made to the dry land during the era.

In addition to showing the position of the sea at the beginning of the era, these marine deposits of early Tertiary times, over the Western States, prove that the Gulf Stream, to which we are now indebted for our mild winters, could not then have had any existence, because the depression of the land required to submerge the Missouri District, necessarily caused the ocean to overflow the Isthmus of Panama, and the effect of this would be that the Atlantic equatorial current would find its way into the Pacific instead of being, as at the present day, deflected to the North. Let us suppose then that the Tertiary era commenced with the level of the ocean something as follows: Over the North of Europe, as well as the North Atlantic basin generally, and also over the New Zealand quarter of the sphere, the land stood in mid-latitudes, say four or five hundred feet more out of the sea than at the present day, whilst in the South Atlantic and North Pacific the land was submerged to a corresponding amount. But along with these circumstances must be placed the other important fact that, up to the close of the secondary era, the North Atlantic enjoyed a warm, if not a tropical, climate. This has been made quite clear by the recent investigations in Greenland. But if, on the one hand, the Secondary era closed with a warm climate, the Tertiary era, on the other, began, as we have just seen, under circumstances which must have brought on meteorological conditions of extreme severity. Because, without the Gulf Stream, the British Isles, as respects climate, would be in the same condition as Tierra Del Fuego at the present day. But, according to my views, our Islands, in early Tertiary times, were not only without the moderating influence of the Gulf Stream, but they stood higher out of the sea. This question of the level of the sea at the beginning of our era cannot be too strongly insisted upon. We must also keep before our minds that in a shifting of the axis, the four quarters of the sphere divide themselves into pairs, the one pair being reciprocal to the other. The following figure is intended to illustrate the great Tertiary oscillation of level:—

Fig. V.



For convenience, the four quarters are here named the North and South Atlantic and Pacific respectively; and manifestly, when the North Atlantic and the South Pacific became areas of depression, the South Atlantic and North Pacific necessarily became areas of elevation. In other words, the two quarters which form the antipodes to each other rose or fell simultaneously, the other two being their reciprocals.

SEC. VI.—PROOFS THAT THE AXIS HAS SHIFTED.

Owing to the fact that there is a margin of early Tertiary formations, and early Tertiary only, round the shores of the N. Pacific and S. Atlantic quarters of the sphere, I have inferred that, at the beginning of the era, the sea over these two regions stood considerably higher than at the present day. Again, from the absence above the level of the sea over the N. Atlantic and S. Pacific quarters of the sphere, respectively, of all early Tertiary rocks, except in places which have been areas of local disturbance, it has been inferred that the dry land was more elevated than at the present day. If we now pass from the beginning to the close of the Tertiary era, we find the level of the ocean in all the four quarters of the sphere just reversed. In the N. Pacific and S. Atlantic, the ocean had clearly retired, because, so far at least as the writer has opportunities of knowing, there is not a streak of *a marine* formation between the margin of the early Tertiary already referred to, and the post-Tertiary deposits which so profusely cover S. America in the one quarter of the sphere, and Eastern Asia and N.W. America in the other. But the ocean retired from the two quarters of the sphere just named only to advance on the S. Pacifie and N. Atlantic respectively. This is proved by the facts, that late Tertiary formations were deposited along the eastern border of N. America; that late Tertiary marine shells have been found in the British Isles 2000 feet above the sea; and, in short, the ocean asserted its dominion to an extent that virtually converted Northern Europe into an Archipelago, and there is evidence of the same synchronous march of events in the S. Pacific. In New Zealand late Tertiary limestones, at a height of 1800 feet, are, according to Dr Hector, overlaid by basaltic rocks, which *were deposited as submarine lavas;* and, as regards Australia, that widely distributed late Tertiary formation known as the Desert Sandstone, rises to a height of 1000 feet.

Now, it may well be asked, to what do such facts point? On the supposition that the axis shifted, the question is free from difficulty, as manifestly the oceanic protuberance in low latitudes must have travelled with the equator; but if, on the other hand, we assume that the axis remained fixed, we are at once face to face with the following startling result—namely, that, over the N. Atlantic basin, the level of the ocean along our coasts rose from being some 500 or 700 feet below the present shore line, till it reached an elevation of at least 2000 feet above it, and all this occurred without affecting the mean momentum. In such a case figures will help our imagination. Allowing for dry land, but, in doing so, it is to be kept in mind that the sea stood 2000 feet higher on the British Isles and on New Zealand respectively than at the present day, we may take the surface of the N. Atlantic and S. Pacific oceans as together equal to 60,000,000 miles; and perhaps the error will not be serious if we assume that the vertical amount of the rise of these two oceans had been 1000 feet. These figures give a disturbing volume of water of 11,393,939 cubic miles. But the rise in the level of the sea in the N. Atlantic and S. Pacific presupposes the withdrawal of corresponding volumes of water from the other two quarters of the sphere; and, so far as the momentum was concerned, this would double the result, giving us 22,787,878; or taking, in round numbers, its equivalent in granite, we get 8,000,000 cubic miles, equal to a table land 2000 miles long, 800 miles broad, and five miles high. And is it to be credited that such an enormous mass could be added to *one quarter* of the sphere without displacing the axis of rotation?

In an oscillation of level due to the shifting of the axis, the vertical effect on the level of the sea increases from the equator to the medial parallels of latitude, assumed to be 51°; and, besides this, there are also two meridian lines of maximum change, as well as two of no change. In accordance with this, we find that whilst the Tertiary sea rose at least 2000 feet above its present level in the British Isles, it did not, on the other hand, exceed 200 along the eastern margin of North America, and similarly the amount of the rise decreased in going east, the marine Tertiary fossils rising to about 60 feet at the southern extremity of the Red Sea; and, on going to the corresponding quarter in the Southern Hemisphere, we find that the sea in New Zealand, which is not far from the meridian of maximum effect, rose to a height of at least 1800 feet, whilst in Australia there are not, so far as I am aware, any marine Tertiary deposits above 1000 feet.

Let us now look at the question from a north and south point of view. The glacial conditions, combined with the subsequent drift phenomena, have masked or rather effaced the evidence of the marine Tertiary deposits in the north of Europe. But in the Red Sea, which passes over about seventeen degrees of latitude, the Tertiary rocks which skirt its margin *are higher at its northern than at its southern end.* Here, at least, the facts are in accordance with the view that the vertical amount of the subsidence increased with the latitude. As might have been expected, the evidence of the post-Tertiary oscillations of level has remained more intact, and hence we find round the North Atlantic basin a series of marine deposits resting on the denuded surface of the drift and glacial clays. In the Gulf States of America these deposits are at an elevation of 400 feet. On "Lake Champlain," I cite from Dana, "the elevation above the present shore line must have been about 460, at Montreal above 450, *above the St. Lawrence*, and in the neighbourhood of Barrows Straits the height is said to be about 1000 feet." Recrossing the Atlantic, exactly the same progression meets us in going north. Messrs Wood and Rome describe the Hessle Clay as warping the whole of Holderness and East Lincolnshire, and reaching elevations approaching 150 and perhaps even 200 feet. As regards Ireland, Mr. Kinahan states that in all places below 250 feet the drift is more or less crushed and sorted into silt, sand, gravel, or shingle. In Banffshire there are marine shells at 350 feet, and stratified drift continuous with the above ascends to heights of 500 feet. Going north to Scandinavia we have the authority of Prof. Kjerulf that since the glacial period the sea has stood at an elevation of 600 feet (and no more) above its present level. Now, contrast this with an instance south of the equator. The inner passage between the Barrier Reef of Australia and the land *grows both wider and deeper with the southing*, and if we are to believe with Darwin that the great coral reef was formed during a subsidence of the land, manifestly this is in strict accordance with the view for which I am contending, and it is difficult, if not impossible, to explain the facts on any other supposition than that the amount of subsidence increased with the latitude. A mere local oscillation is out of the question, because the reef extends for a thousand miles in a north and south direction. Nay more, the same law extended to New Caledonia, for we are told that for many leagues the reef on the west coast of that island is eight miles from the shore, but *near the southern end* the distance increases to sixteen miles.

SEC. VII.—CAUSE OF THE SHIFTING OF THE AXIS.

It will be remembered that the Secondary era closed with a warm climate over the North Atlantic, and that the existing era began with the British Isles higher out of the water, and without the moderating influence of the Gulf Stream to temper our northern winters. Further, it must not be forgotten that, according to the views advocated above, the British Isles were then farther from the equator. And when we take these conditions into account we have everything necessary to produce Arctic conditions of extreme severity. When considering the question of the deposition of snow and the formation of land-ice, it is of the utmost importance to bear in mind that it is the *temperature of summer which determines the line of perpetual snow;* and one of the consequences of this law is, that snow or ice never accumulates over continental areas because there the extremes of temperature are great. In insular positions, however, the case is different: there the moderating influence of the sea produces a double effect. On the one hand, it raises the temperature of winter, but not to an extent that prevents the fall of snow, whilst, on the other, it depresses the temperature of summer, so that the heat is not sufficient to dissolve the snows of the previous winter; and hence, such accumulations of land-ice as we see in Greenland, and its absence in the same latitudes in Asia, and on the continent of North America. Now, keeping these circumstances in view, it will be seen that at the beginning of the Tertiary era the North Atlantic basin was exposed to conditions in every way favourable to an accumulation of snow and ice. But it can hardly be necessary to remark that all the snow and ice formed simply meant so much solid matter added to one quarter of the sphere at the expense of the other three, or rather the other two; for, as presently to be explained, the same conditions existed in the South Pacific as in the North Atlantic. But snow and ice could not accumulate over these two quarters of the sphere without increasing the momentum, and the inevitable result must have been to draw the equator farther north on the meridian of

Greenwich, and, of course, farther south on the meridian of New Zealand.

SEC. VIII.—THE TERTIARY ERA INTRODUCED BY GREAT OCEANIC MOVEMENTS.

It is well known that the junction between the chalk and the basement bed of the Tertiary strata bears evidence of tumultuous movements of the ocean; and I think there are some considerations which go to show that the direction of the movements was from the south. Excepting perhaps an opossum, which may have been introduced from America, all the Marsupials over Africa, Europe, and Continental Asia, appear to have been destroyed at the crisis in question. This disappearance of the Marsupialia from the northern hemisphere becomes the more remarkable when we bear in mind that so many genera of the same group survived in the south. Evidence afforded by the geographical distribution of the Struthious birds, is to the same effect. True the Ostrich survived in the northern hemisphere, but then it is to be borne in mind that Africa projects far south of the equator, and even against this solitary form which extends into the north, we have of the Struthionidæ in the south, two species of the Rhea in S. America, the Cassowary of the Eastern Archipelago, the Emeu of Australia, the Mooruk of New Britain, and two species of the Kiwi-Kiwi of New Zealand, and of the Brevipennates that have wings, too rudimentary for flight, we have the Dodo and Solitaire of the Mauritius and Rodriguez, as well as several of the extinct forms found in the superficial deposits of New Zealand and Australia.

In support of this view that the oceanic currents which began the Tertiary era were from the south, there is even the evidence of superposition. Prof. Winchell has obtained proof that what is known in the State of Alabama as the "Rotten Limestone" (Upper Cretaceous) "has, in many cases along the junction of the upper and lower strata, *been moved northward*, over the clayey and sandy region pertaining to the lower Cretaceous."

The same view receives strong support from the fact that at the close of the Secondary era the Flora of Australia and America were transported to Europe.

"Out of the 180 species from the Eocene beds of Haring," says Dana, "55, according to Ettingshausen, are Australian in type, 28 East Indian, 23 tropical America, 14 South African, 8 Pacific, 7 North American and Mexican, 6 West Indian, and 5 South European. The resemblance to Australia consists not merely in the number of related species, but in their character."

The facts contained in this extract convey a meaning which cannot be easily misunderstood. For such a meeting in Europe of the Flora of the southern hemisphere is, as appears to me, quite inexplicable on any other supposition than that great abnormal movements of the ocean transported the various species across the equator; and, in considering this question, it must also be remembered that at the beginning of the Eocene period the ocean occupied an entirely new level from what it had done during the deposition of the chalk. It ought also to be remembered that there is a synchronism between these oceanic movements and the upheaval of the Andes and Rocky Mountains. In other words, the strata of the Secondary era were upheaved with the volcanic and older rocks forming the mass of that great range of mountains extending from Patagonia in the south, to the Arctic circle in the north; and this being so, it need excite no surprise to find that the contemporaneous movements of the Ocean were of the most comprehensive character. After showing that the relations between the basement bed of the Eocene and the uppermost Cretaceous formation, are the same both in America and Europe, T. A. Conrad, Esq., in a paper in the American Journal thus concludes: "So far in Europe, I believe, no authentic account has been published of any fossiliferous beds between the lower Eocene and the Cretaceous strata that would indicate a transition period to have intervened, and so in Utah, Idaho, and Wyoming the same remarkable condition is apparent, as it is in the Eastern States, in England, Spain, India, on the banks of the Nile, and in South America, which proves that *the revolution which closed the Cretaceous era was co-extensive with the globe*, and destroyed the life of the period."

Taking these significant circumstances into consideration, we do not require either to strain the facts or to go beyond the bounds of reasonable probability, to conclude that Victoria land, like Greenland, began the era without its coat of ice, and having been at the time, on my view, also, like Greenland, farther from the equator, we have thus in the South Pacific a great region of gathering ground for ice and snow corresponding to that in the North Atlantic. But, on the supposition that the ice and snow accumulated simultaneously on both these quarters of the sphere, the manifest effect would be, as stated above, to draw the equator to the north in the Atlantic, and to the south in the Pacific.

But another force that necessarily acted in the same direction remains to be noticed. One of the obvious consequences of the Oceanic movements adverted to above, and the emergence of so much dry land from the Cretaceous sea, was that at the beginning of our era the continents must have been virtually without a system of drainage. Owing to this and to the unexcavated river channels enormous volumes of water must have been cut off from the sea, and collected in what may be called the dry land part of the sphere, and we will not obtain anything like an adequate idea of the influence which the water thus isolated exercised on the momentum, unless we keep in mind that there are wide areas in Asia, including the basin of the Caspian, Aral, and valley of the Jordan, now below the level of the sea; and, moreover, there are, over the eastern table-land, wider areas still, which, though above the level of the sea, yet form great depressions without any outlet. Now, at the time to which this discussion refers, probably every one of these great concavities was filled with water to the brim, and that the water enclosed in this way formed no mean factor in the question will be at once seen when we recall to our minds the fact that the great dried up lake Shamo alone embraces an area of 300,000 square miles, and that many of the rivers that fall into these depressions are of considerable magnitude, some of them having numerous affluents and courses of several hundred miles in length. That the fixing of such enormous volumes of water within impassable barriers affected the momentum cannot, as I apprehend, be disputed; and this is not all, because the abstraction of such a mass of water from the ocean necessarily depressed the mean level, and as the land and water are so very unequally distributed both as regards latitude and longitude any appreciable decrement of this kind would be like taking so much weight from one scale and adding it to the other.

When, however, the conditions proper to the new era became established, evaporation and the deepening of river channels must have gradually drawn off the water from those land-locked seas and lakes of Eastern Asia, and this necessarily reduced the momentum over those regions, whilst it increased it over the ocean generally. To sum up, whilst the accumulation of snow and ice already discussed, drew the equator farther north in the N. Atlantic, with a reciprocal southing in the Pacific, this gradual withdrawal of inland seas from Eastern Asia added to the amount of the displacement; and I need hardly add that, from my point of view, we will fail to study the question to advantage unless we dismiss from our minds all our preconceived notions about the solid mass of protuberant matter on the equator. Doubtless, as I have already pointed out, there is a sufficient excess of solid matter between the tropics to maintain that permanency in the axis which we at present enjoy; but I have shown, as I hope, that this mass has nothing of the dimensions usually taken for granted, and that it is quite insufficient to form an equipoise to the great transposition of the ocean and accumulations of ice that have unquestionably taken place during the Tertiary era. For the above reasons, I feel myself justified in proceeding on the assumption that the displacement of the equator actually took place; and I will, therefore, now address myself to the question of showing that the formation of the Parallel Roads of Glen Roy was one of the necessary consequences of the depression of the land which ensued.

SEC. IX.—THE PHYSICAL GEOGRAPHY OF LOCHABER AT THE TIME.

The sides of Glen Roy, as well as those of the neighbouring valleys, are steep, and, in earlier times, they must have been more so, for, owing to the weathering of the rocks, and to the disintegrating influence of severe winters, the sides of the glens are covered with a thick coating of angular detritus, which, according to Sir John Lubbock, is built up with a slope not far from the angle of repose, that is, its surface is at as sharp an angle as shingle can lie on a declivity. But I think this is probably going too far, because shingle on a hill side may be set in motion by any or by all of the four following forces: (1) A stone falling from the parent rock rolls down the hill, carrying others with it; (2) the rains, while they carry down the finer sediment, at the same time undermine the larger pieces; (3) snow slips down in masses producing the same effect on a larger scale; and

(4) storms are more influential still in setting the shingle in motion over considerable areas at the same time. Thus it will be seen that the shingle is carried down by its own inertia, as well as by gravity, and so we may safely conclude that it comes to rest at an angle somewhat less than that of repose. Well, keeping this state of the valley slopes in view, let us now consider the effects which the action of the sea necessarily produced on this layer of loose shingle, as it, year after year, crept up the sides of the Lochaber hills. In air, as just remarked, shingle descending a mountain is carried forward by its own momentum, as well as by the force of gravity; in water, on the other hand, the momentum is destroyed by the resistance of the medium; and, for this reason, we ought to expect a slope of shingle formed in water to be steeper than one formed in air. Hence, when the sea began to act on the hillside, it is clear that the detritus rolling down from above would form a sharp brow, along a line just below the action of the breakers at low water, and this re-arrangement of the detritus would produce a new slope at a somewhat steeper angle than the hillside generally. In such circumstances there were obviously two sources of detritus; first, but least important, the products of the weathering and annual disintegration of the rocks; and, secondly, the masses of the shingle, previously accumulated, brought down by the undermining action of the breakers on the shore line. In this way the building up of the new layer would keep pace with the rise of the level of the sea, and the breadth of the layer would be determined by the supply of detritus, and this again by the rate at which the land subsided. But I must retrace my steps a little.

A tide entering a contracting channel, partially at least, gains in vertical range what it loses in lateral dimensions; and, for the purpose of my argument, it is of great moment to remember that the increment of the range is in the high water alone, that is, there is no corresponding depression of the low water mark. Suppose, for instance, the mean along the coast generally to be 10 feet, but owing to its entering a contracting channel the tide rises to a height of 16 feet, then in such a case the extra 6 feet is all in what may be called an abnormal rise of the high water, the low water remaining all the while on a level with that of the sea outside the channel. At the present day there are three things which tend to prevent an exaggerated tide at the head of Loch Linnhe. First, the island of Lismore, standing as it does in the mouth of the loch, breaks the force of the tidal wave; secondly, between Lismore and Loch Leven, a part of Appin juts out; thirdly, the current is obstructed by the flat triangular piece of ground projecting at Corran Ferry; and, fourthly, a considerable portion of the flood tide which passes Corran now flows into Lochiel. But, on the marine hypothesis, the lowest terrace was formed when the sea stood at 850 feet above its present level, and hence, the obstructions just referred to, did not then exist, and instead of the current passing on into Lochiel a second branch of the same tide must have run just in the opposite direction, passing through what is now Loch Shiel, Glenfinnan, and Loch Eil, meeting the Loch Linnhe current in the basin of Lochaber. But beyond the basin of Lochaber there lay the basins of the Roy and the Spean to be filled up, and therefore the flood tide now comprising the united currents moved onwards into the vertex of the triangle at the mouth of the two glens. But the tide could not flow into a contracting space of this kind without a very considerable increase in its height, and this increase in the height necessarily raised the mean level of the sea in Glen Roy. To place this point in as clear a light as possible, let me first make the supposition that there was no exaggeration of the tides at the mouth of the glens; or, in other words, that they ebbed and flowed through the same vertical space as the tides along the west coast generally—say 10 feet. In such circumstances the mean level in the glens would be exactly the same as the mean level outside, because the height of the flood tide would fix the volume of water passing in through the entrance each tide, and the low water line would similarly fix the quantity flowing out. Suppose again, an exaggerated tide—say to the depth of six feet—to be superimposed on the normal one just described. The tide of our first supposition rose during the flood 10 feet, but the rise now will be 16 feet; that is, a current six feet deeper than formerly would flow into the glens at high water, and mark the effect. The entrance being small, relatively to the size of the basins inside, the ebb would set in before the waters in the glens rose to the level of that outside. Say the actual rise was 13 feet, and we are then in a position to see the effect of the first exaggerated tide entering the glens. The current during the flood corresponded to a rise of 16 feet, but the force of the ebb tide was determined by a depression of only 13 feet, hence the flux exceeded the reflux, the difference going to raise the mean level of the sea inside the glens; and, manifestly, the mean level would go on increasing until the volume of water flowing out at each ebb tide equalled the flood; in other words, till the average level of the sea in the glens became a mean between the normal low water and the exaggerated high water outside. I have, for the purpose of illustration, named six feet as the extent of the exaggeration of the tide, but there are good reasons for concluding that it was more. We know, for instance, that the range of the spring tides in the Bristol Channel is about 18 feet at the entrance, 30 feet in Swansea Bay, and by the time the tide runs up the still contracting channel to Chepstow, the difference between the high and low water sometimes reaches 50 feet. We have just seen that the tide not only then flowed uninterruptedly up the contracting channel of Loch Linnhe into Lochaber, but another branch of the same tide reached the same basin through Loch Eil. Glen Roy and Glen Spean may be said to have a common entrance, and that entrance is at the vertex of a triangle to which the hills Meal nan Luath and Sron nam Ba form the one side, and the Ben Nevis range the other. A tidal current running into a converging space of this description, is not only seeking its own level, but it is propelled forward by an enormous horizontal column similarly in motion, and the consequence is, as just remarked, that the tide gains in height what it loses in breadth. But similar causes produce similar effects; and, therefore, unless we are prepared to reject all that is known to be elsewhere the law of tides, we must take it for granted that the tide, during its progress, first up the narrowing area of Loch Linnhe, and thence into the vertex of the triangle at the mouth of the glens, rose far above the mean level. But, whatever was the amount of the exaggeration of the tide at the common mouth of Glen Roy and Glen Spean, it is manifest that the water flowing into these glens each tide, was fixed by two factors, the one being the depth of the current at high water, and the other the breadth of the channels, and this limitation is essential to the solution of the problem.

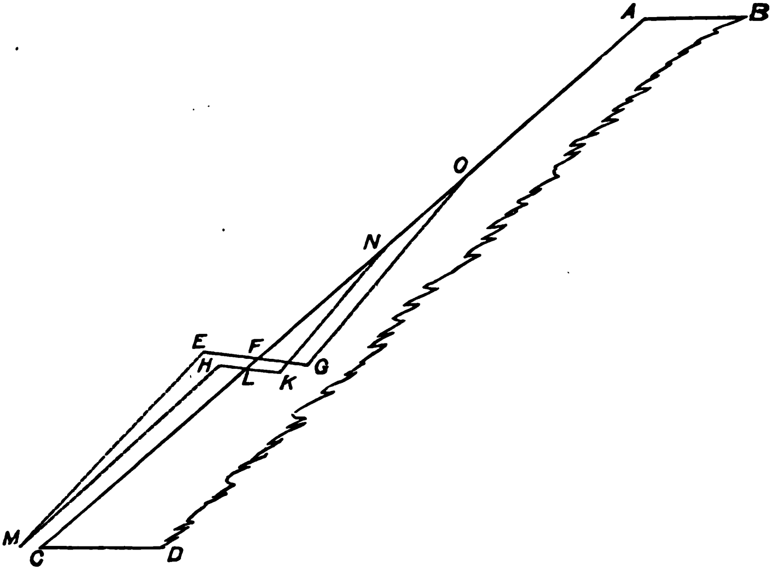
SEC. X.—THE EXPLANATION OF THE PHENOMENA.

In explanation of the correspondence between the levels of the "roads," and the respective cols, Prof. Nichol suggested that there was a difference of level between the East and West coast, and owing to this the interference of the cols gave a check to the gradual fall of the level of the sea in Glen Roy, and the protracted action along the high water line which this check produced formed the terraces. But whilst the learned Professor's suggestion is, I am satisfied, in the right direction, yet it is wrong in two particulars. Instead of the terraces having been formed during a gradual emergence of the land, they were, according to my view, formed during its subsidence, and, instead of the check having been caused by the difference between the levels of the two seas, its cause is to be sought in the position of the cotidal lines round the British Isles. At the present day it is low water in the Moray Firth at about much the same time as it is high water in Loch Linnhe, and, supposing the same difference to have existed at the time the "roads" were formed, it is manifest, that when the sea rose to the summit level of the Pass of Makoul, which, between Inverness and the Frith of Forth, is the lowest point of the dividing ridge between the two watersheds, a regular interchange of the tides through this pass or col would take place. Keeping this in mind, and also what I have already urged, that Lochaber was then a sea-basin, into which the tides flowed through Loch Linnhe and Loch Eil respectively, and from this basin they again flowed on through a narrower channel still, into Glen Spean and Glen Roy; I say, keeping these circumstances in mind, we are, I think, in a position to understand how the interference of the cols produced the "roads." Suppose the sea to have gradually risen till the high water line was on a level with the lowest col. After this point was reached, the tides, over what is now the Loch Laggan area, could not have risen to their normal height, for, as it was high water at the one side of the col when it was low water at the other, the greater proportion of the tide entering at the mouth of the glen would flow over the col and join the ebbtide in what is now Strathspey. True, at first the overflow would be shallow, but as the depression of the land proceeded it would acquire a depth and breadth not far short of that entering at the other end. Before the two seas met the filling up of the valley of the Spean by the flood tide, stopped to a considerable extent the onward movement of the crest of the tidal wave, thereby increasing its vertical range over that area at the mouth of the glens which I have described, as the vertex of a triangle. But after the interchange of the tides through the Pass of Makoul had become fully established, the tide would simply flow through Glen Spean into the North Sea, *hence the banking-back effect produced by the filling of the Loch Laggan area each flood tide would cease.* We have already had occasion, more than once, to remark that a tide flowing into a converging space, gains in height what it loses in breadth, and the reason is obvious; the horizontal column, which may be almost any length, is carried forward by its own momentum, as well as by the force of gravity, and consequently when a tide enters a narrowing channel, which finishes in a *cul de sac*, it rises at the extremity far above the mean level. Now, when the tide began to flow through the Pass of Makoul, Glen Spean ceased to be such a *cul de sac*, and the manifest effect must have been to depress the level of the exaggerated tide at the common mouth of the two glens. To put the case otherwise, instead of the level inside of the entrance to Glen Spean rising to the high water line outside, its actual height would be determined by the volume of water flowing through the outlet at Makoul, with the obvious effect that the depth of the current entering Glen Spean at, and near high water, would be greatly increased. But the carrying off of such a large volume of water to the opposite sea necessarily relieved the pressure of the horizontal column pressing towards the common mouth to the glens, *and this reduced the vertical range of the tides.* Now Glen Roy and Glen Spean having had a common entrance from the larger basin of Lochaber, this depression of the high water line outside, must have caused a corresponding depression in Glen Roy, and it is to be borne in mind that the mean level in Glen Roy was not the mean level of the ocean generally, but the mean between the exaggerated high water line outside the mouth of the glen, and the normal low water line. Hence the interference of the col produced a twofold effect; by its indirect influence, the tide, as we have just seen, did not rise so high at the mouth of the glen, and consequently, the current running into it each flood, must have had less depth and velocity, and as the normal low water outside remained relatively unchanged, the evident effect must have been to depress the mean level in the glen; secondly, the fact that the current running into the glen each flood possessed less depth and velocity clearly implies that each separate tide did not rise so high inside the glen as formerly. But such a depression in the high water line would *appear simply as a halt in the secular rise of the sea.*

But a halt of this kind could not have occurred without causing a protracted action of the breakers along a line on a level with the col. We have already seen how, whilst the high water mark was, year after year, ascending the hillsides, the shore action of the breakers must have built up a new layer of shingle at the expense of that which came down from above. Now what the halt did was this: by cutting deeper into the shingle it broadened the plane between the high and low water lines in the direction of the land; and the additional detritus brought down from above and by the action of the waves, built up with a steeper slope below the low water line, broadened it in the direction of the sea. In my opinion this broadening process during a relative halt in the rise of the high water line, affords an explanation of the manner in which the Parallel Roads were formed.

Hitherto I have only considered the effect of a depression of the high water mark; but, on my hypothesis, our conception of the phenomena will be incomplete unless we also take into account the effect produced on the low water mark as well. Unlike the case of the high water, the effect of the interference of the col must have been to accelerate its rate of rise. Because, as the ebb tide flowed out of Glen Spean, it would be replaced by the flood tide through the Pass of Makoul from the North Sea, and this would prevent the ebb from falling so low as it otherwise would have done at the mouth of Glen Roy. Now, a moment's consideration must satisfy us that the accelerated rise of the low water mark could not have been altogether uninfluential in giving to the "roads" their present form. The halt in the rise of the high water line afforded the breakers time to cut deeper into the hillside, with the obvious effect of bringing down the detritus in larger quantities. The waves again spread out this detritus into a slanting plane, bounded at its upper edge by the line marking off the high water mark, and, at its lower one, by the line where the breakers at low water ceased to act. The latter line of course formed what may be called the brow of the plane, and over this brow the back-draught of the waves washed the loose detritus. For this reason the plane included between the high and low water lines would not only be broadened, but it would be made more horizontal, because, owing to the fact that the low water line was rising, whilst the high water line remained relatively stationary, less detritus would be washed over the brow than formerly, and this would produce a building-up effect along the lower edge of the plane, and it is doubtless to the operation of this cause that the "roads" owe much of their symmetry. But perhaps the diagram on the following page will show the *modus operandi* better than words.

The waving line *BD* represents the solid rocks in place on the side of Glen Roy, and *AC* the surface slope of the talus before it was acted upon by the rising sea. *K* and *H* mark the high and low water lines respectively, at the time when the tides first began to overflow the lowest col. The extent to which the breakers ate into the detritus during the uninterrupted rise of the sea is shown by *LK*, whilst *NKL* represents the detritus brought down from above. But when the interference of the col placed an arrest on the rise of the high water line, the breakers are supposed to have found their way to *G*, with the effect of bringing down an additional quantity of detritus, represented by *OGKN*. After the low water line reached the summit level of the col, both it and the high water line of course rose at the same rate as the mean level; but, in altered circumstances, instead of reaching the brow at *E*, the detritus would probably not reach *F*, because there was now all the vacant space *OGF* to fill up, with probably very little detritus within reach of the action of the breakers, and hence, the platform *EF* would sink into deep water as a protruding terrace.



To avoid an exaggerated notion as to the time required to form the terrace, we must give some attention to the law that, in such a case, fixes the quantity of shingle coming down from above. As the slopes in the figure represented by the lines *OG*, *NK*, and *EM*, are all supposed to be near the angle of repose, the lines themselves may be considered parallel to each other, and hence a very simple calculation shows, that, supposing the first square foot undermined by the breakers, to have brought down 100 cubic feet of shingle, an indentation of 10 feet must have brought down 10,000 cubic feet. But 10,000 cubic feet would form just such a triangular section as is indicated in the figure by *EMH.*

In my argument with respect to the exaggerated tide, at what I have called the common mouth to the two glens, I have taken my grounds from what is known of the laws and the facts of the tides at the present day, and, until recently, this was all that was practicable and all that could be reasonably expected, but the publication of the results of the Ordnance Survey has placed within our reach a number of facts that cannot be well either misinterpreted or misunderstood. As is well known, the lowest of the three terraces can be followed for a short distance outside of Glen Roy into Glen Spean. Opposite Coirecoille, on the Ben Nevis side, the height is only 848 feet above the level of the sea, but as the valley of the Spean going east rapidly narrows, the height near Chlinaig reaches 862 feet, and following the same terrace to Monessie, we meet with a very instructive fact. At Chlinaig the valley keeps contracting, but at Monessie it widens into the channel of the Burn of Bruach, and the result is just what might have been expected, there is a depression of 6 feet in the level of the terrace. Again, when we go to the north side of Glen Spean, we find, in going eastwards towards the mouth of the glen, the following progression marked on the Ordnance map—857, 860, and 872 feet, the last level being in the saddle formed by Meall nan Luath and Maol Ruadh. It is worth noticing that these figures show that the terrace on the north side of Glen Spean is higher than that on the south side, and this, again, is just what ought to have been looked for. The tidal wave is primarily set in motion by gravity, but when once in motion the inertia of the water tends to propel it in straight lines, and hence the direction of the current at any given place is determined by the resultant of two forces—the one being the horizontal movement of the current, and the other that property which causes a fluid to seek its own level; and, as this consideration cannot be too strongly insisted upon, I will endeavour to make my meaning clear by a reference to the tides of the English Channel. The flood approaches the Scilly Isles from the west, and, accordingly, the range of the tide on the north-west of Cornwall, against which the horizontal column impinges, exceeds that on the south-east coast by about 10 feet.

In proceeding up the channel, the tidal wave is obstructed by the Channel Islands and the projecting province of La Manche, and the consequence is that a part of the current is deflected into the Bay of St. Malo, where the tides become greatly exaggerated, sometimes reaching a vertical range of 50 feet, whilst the other part arrested on its one side is drawn round, forming a sort of eddy to the west of Cape de la Hague, and hence, the range at Havre is 4 feet more than at Portsmouth, on the opposite side of the channel. Beyond these points there is no obstruction, but owing to the trending of the coast, the current in its onward course to the Straits of Dover, now impinges more upon the English than the French side, and, consequently, we find the range of the tide at Beachy Head 3 feet more than at Dieppe, at Hastings 6 feet more than at Boulogne, and at Dover 8 feet above Calais: and the agency of the same law is even more manifest in the tides of the Irish Sea, for along the whole west coast of England, from the Land's End to the Solway, the range of the tides is double that along the east coast of Ireland. Now, applying the same law to the question under discussion, it is clear that the horizontal column of water moving up Loch Linnhe, and probably slightly deflected towards the east by the accession of the current through Loch Eil, must have impinged on the north side of the Lochaber basin, and this, reasoning from all analogous cases, would increase the range of the tides. The difference of the levels between the north and south sides is about 8 feet; but I do not attribute the whole of this to the cause just explained. From the contour of the surrounding mountains, waves of any considerable size could only have been generated by a south wind, and for this reason the breakers were necessarily heaviest along the north side. In other words, when, during storms, the north side was the lee shore, the waves were always high, whereas, when the south side became the lee shore, the want of sea-room to windward prevented the formation of any but waves of a small size. In such circumstances the breakers acted against a higher line on the north, than on the south side of the valley, and this could not have been uninfluential on the height of the terrace.

THE MIDDLE TERRACE.

Everything advanced in explanation of the lowest terrace applies equally to the middle one, but, in order to see this clearly, we must take into account the changes in the physical geography, produced by a further depression of 212 feet. To any one travelling along the public road, the Pass of Makoul appears narrow, with a steep hill projecting into the valley on the south-side. But its steepness and nearness to the observer makes the hill appear higher than it really is. By a very rule-of-thumb method, I satisfied myself that the height above the level of the valley does not exceed a 100 feet Hence, before the high water mark reached the level of the second col, this apparent obstruction must have disappeared in deep water, with the effect of doubling at least the breadth of the strait; and this was not all, for, so far as I could judge, without actual measurement, the height of the watershed, between Loch Laggan and Loch Cruachan, cannot exceed 200 feet; and, if I may be allowed to take it for granted that this estimate is not far from the truth,' then, in this case, we would have a second strait with regular interchange of the tides through what is now Glen Shirra. Thus, a depression of 212 feet would open up a free and comparatively broad passage between what are now the Strathspey and Loch Laggan areas. At the west end of Loch Laggan we have, on the other hand, the narrow gorge between Monessie and Murlaggan walled in, as pointed out by Professor Nichol, by steep hills on both sides, and consequently such a depression would add little to the breadth of the strait. In a word, when the sea level rose to the second col, there must have been this state of things. Owing to the freer ingress of the tides from Strathspey, the actual dividing line between the two watersheds became transposed; that is, instead of the narrowest part of the strait being, as formerly at Makoul, it was now at Murlaggan, where the narrowness of the strait placed obvious restrictions on the volume of water passing through it at each flood tide. With this we must take into account that owing to the gentler slope of the west side of Creag Dhubh, the tide had a wider entrance to Glen Roy as far as Glen Glaster, but beyond this, the conditions remained very much the same, except that a narrow passage into the former glen must have been established on the west side of Bohuntine Hill. In these altered circumstances it is clear that the high water mark at the mouth of Glen Glaster would now determine the depth of the column of water flowing into Glen Roy. Wherefore, when the overflow through the middle col depressed the high water line in Glen Glaster, it is manifest that this would reduce the vertical range of the column entering Glen Roy. But a decrement of the kind necessarily, not only reduced the range of the tide, but also the mean level of the sea inside the glen. These two effects combined would place an *arrest* on the secular rise of the high water line, and this relative halt formed the terrace in exactly the same manner as the effect of the interference of the lowest col produced the lowest terrace. A difficulty may, however, here occur with respect to this second shortening of the range of the tides. After the first depression took place the strait through the gorge at Murlaggan must, it may be objected, have gone on broadening with the effect of further depressing the high water mark at the mouth of the glens. But the answer to this is that co-temporaneously with this broadening of the Murlaggan strait, the channels through which the tide entered the basin of Lochaber were also becoming deeper and broader. In this connection it is also worthy of observation, that the levels furnished by the Ordnance Survey, prove that the exaggerated flood-tide was higher on the Glen Roy than on the Glen Spean side of the area which I have described as the vertex of a triangle; and hence an outlet to the waters impinging on the north side would produce a depression quite irrespective of anything that took place on the south side. But the influence of another law must not be left out of account. Friction retards water in motion, and a well-known consequence of this is that the part of the tide which skirts a coast, always lags behind that in deep water. Now, on my hypothesis, by the time the sea rose to the level of the middle col, the channels, namely Loch Linnhe and Loch Eil, through which the tides from the west flowed into the Lochaber basin, had not only become broader, but they had become 212 feet deeper than when the lowest "road" was formed. But with broader channels and deeper water, the tide must have entered Lochaber in a greater volume, and with a greater velocity, and for this reason we may safely take it for granted that by the time the level rose to the middle col, the vertical range of the tide had become quite as exaggerated as it was before the interference of the first col, and it can hardly be necessary to observe, that the return flood-tide from the North Sea through the middle col would prevent the low ebb at the mouth of Glen Glaster from falling to its normal level, and this again would accelerate the rise of the low water mark in Glen Roy, exactly in the same manner, and with the same effects, as occurred in the case of the lowest col. This view of the origin of the middle terrace appears, as in the case of the first, to be borne out by the facts. When discussing the lowest terrace, it was shown that the interchange of the tides through the pass of Makoul depressed the high water line at the mouth of the glens, and as a consequence of this the terrace can be traced for a short distance down the Spean beyond its junction with the Roy. Similarly, the middle terrace extends for a short distance down the Roy beyond its junction with Glen Glaster, but it is altogether absent from Glen Spean and from the south side of Bohuntine Hill, and the only trace of it on the west side of the hill is at the extreme north-west corner, where, from its situation, any oscillation of level must have been in perfect sympathy with that in the basin of the Roy.

THE HIGHEST TERRACE.

The explanation of this "road" is free from difficulty. From the steepness and contour of the hills, a further rise of 80 feet could not have seriously disturbed the relative positions occupied by the land and water in the neighbourhood. True, the narrow straits at Murlaggan and the head of Glen Glaster, had, during the rise, become broader in conformity to the slopes of the hill sides, but, as a set off to this, the corresponding widening of Loch Linnhe and Glenfinnan must have allowed free access to the tides from the west. We have seen that the effect of the overflow through Glen Glaster was to diminish the volume of the tide entering Glen Roy, but to avoid misapprehending the facts, we must remember that before the sea rose to the third col, this was probably counterbalanced by the broadening of the passage on the west side of the Bohuntine Hill. Keeping this in view, we may describe the conditions under which the third terrace was formed, in something like the following terms: The tides entered Glen Roy by two narrow channels, the west one being narrower than the other; and the narrowness of the channels placed a limit on the volume of water ebbing and flowing through them each tide. Hence, when the two seas met at the col—it being high water on the one side, when it was low water on the other—the effect of the interchange was, on the west side, to lower the high water mark, and to raise the low water mark, just in the proportion which the breadth of the col bore to the sum of the breadths of the two channels at the foot of the glen; and this again must have brought with it all the consequences with respect to the curtailment of the range of the tides, and the depression of the mean level, already explained in the case of the two lower terraces. However, it wi perhaps be convenient to reiterate what I have already advanced as to the effect of the interchange of the tides on the level of the sea at the mouths of the glens.

The high water, it will be remembered, flowed through the gorge at Murlaggan, also through the still narrower strait, known as the second col. It will also be remembered that, owing to the contour of the hills Meall Dhoire and Creag Dhubh, the sea, by the time it reached the summit level between Glen Roy and Glen Spean, had a comparatively broad channel up the former glen *as far as Glen Glaster.* At this point, the tidal current divided into two branches, the one turning at right angles into Glen Glaster, whilst the other moved onward into the basin of Glen Roy. Unless, however, we keep before our minds that the law determining the force and volume of the former current is quite different from that of the latter, we will altogether fail in getting a correct view of the phenomena. By providing an outlet to the tide pressing into the lower entrance to Glen Roy, both currents acted in preventing the high water line from rising to the height which it otherwise would have reached. At first flood, both currents must have been very much alike in kind, but here the similarity ceased. Because, owing to the overflow to the opposite water-shed, the Glen Glaster current deepened with the rise of the tide, or, in other words, its capacity for relieving the pressure at the mouth of Glen Roy attained its maximum at high water, whereas the branch flowing into Glen Roy simply raised the level of the sea in the basin of the glen, and this filling up process must have kept step with the rise of the tide outside. It will thus be seen that whilst, on the one hand, the depth and consequent volume of the current running at high water through Glen Glaster, were in proportion to the whole range of the tide; on the other hand, the depth of that running into Glen Roy was only in proportion to the height by which the level of the sea outside exceeded that inside the glen.

When, however, the sea rose to the level of the col at the head of Glen Roy, the flood tide instead of as formerly filling up the basin of the glen, passed through to Glen Spey, and the reciprocal action of the ebb and flood once established, the range of tide in the glen would be shortened and the mean level depressed. These combined effects would appear as a halt in the secular rise of the high water line, and during the halt, the breakers would form a terrace in the same manner as has been explained in the case of the two lower terraces.

Here again, as in the case of the other terraces, the facts are in unison with what might have been antecedently expected from the hypothesis. In order, however, to see this, it must be borne in mind that the sea, when at the level under consideration, had free access into Glen Roy, *as far as Glen Glaster*, but between this and the basin of the glen, there was the contracted gorge between Bohuntine Hill and Leana Mhor; and hence, we ought not to find the effects of the depression extending down the glen much beyond this point, and neither indeed do we. For, whilst the terrace is continuous on both sides of the gorge in question, it disappears altogether where the glen opens into Glen Glaster. Indeed, so clearly is the terrace truncated at this point, that the glacialists, to explain the circumstances, feel it necessary to fill Glen Glaster with a glacier; but the explanation that the terrace was cut short by a glacier embankment, extending across the valley, is to me, not credible. Other reasons might be given, but there is one that appears sufficient in itself, namely, that the basin of Glen Glaster is neither of sufficient elevation, nor does it afford a wide enough gathering ground to breed a glacier, which to answer the purpose must have been at least 800 feet high.

THE GLEN GLUOY "ROAD."

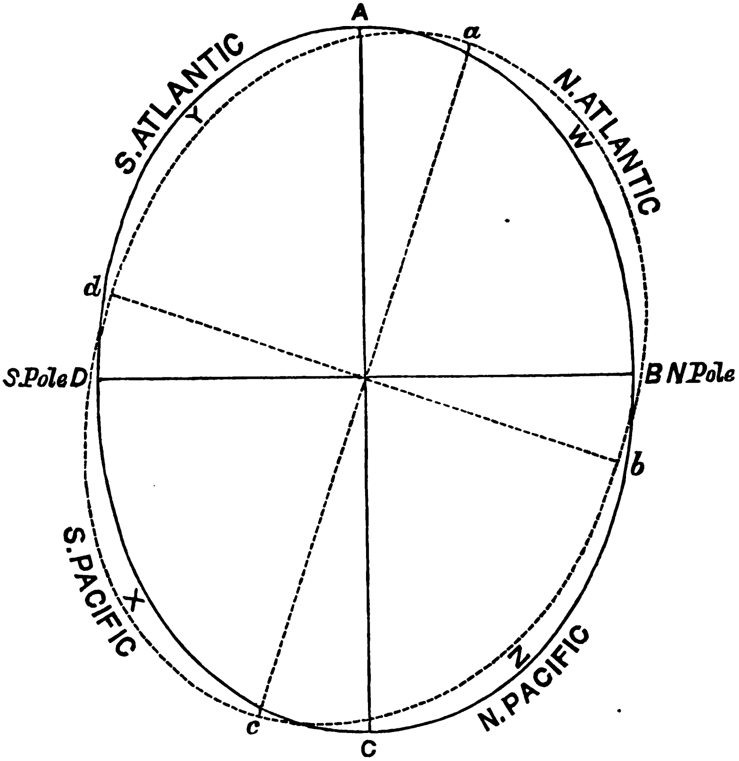
Of all the terraces, the explanation of the one in Glen Gluoy is the freest from difficulty. The valley is deep. and throughout its whole course, gradually contracts. In ascending an inlet with such an outline and soundings, the height of the tide must have increased, and we have only to call the Ordnance Map into court to prove that the question of a considerable rise does not depend upon mere theory. At the most westerly point to which the terrace can be traced, its altitude is 1162 feet, and on going further up the glen it rises to 1165, and further on still to 1173, at this figure it remains constant for some distance, but on approaching the col it falls off again to 1164 feet. That a law runs through these levels cannot, I think, be disputed, and they strictly correspond with what is known of tides at the present day. For instance, the mean range of the tide in the Thames is 4 feet more at Deptford than at Sheerness, but it falls off again before reaching London Bridge. But I need not dwell on a fact so well known, and, as similar causes produce similar effects, I will now assume that the range of the tide did increase in ascending the glen in accordance with the levels ascertained by the Survey. But if, on the one hand, the tide rose above the mean level in Glen Gluoy, in Glen Roy, on the other hand, the tide, owing to the opening of the strait into Glen Spey, could not rise to the ordinary high water level. Hence, during the gradual depression of the land, the high water mark must have reached the summit level of the col much sooner, on the Glen Gluoy side, than on that of Glen Roy, and for this reason the flood tide would flow from the former glen into the latter. To see the effect of this on the level, we must remember that a tide entering a *cul de sac* like Glen Gluoy becomes exaggerated from two causes: First, the converging sides cause the current to gain to some extent in height, what it loses in breadth; and, secondly, when the tide reaches the head of the inlet it produces a banking-back effect on the horizontal column still pressing forward. When, however, the tide at high water began to flow through the col, of course the banking-back effect ceased, and the volume of water that otherwise would have gone to swell the tide in Glen Gluoy, now passed into the basin of Glen Roy. In other words, the flood tide, instead of rising in the glen, till its range became exaggerated, merely passed through it as a current. This caused a halt in the rise of the high water line, and the halt again produced the terrace.

I have had occasion to point out that the levels generally are such as might have been antecedently expected from the effect of tides running into a converging space; but in studying the circumstances bearing on the level at any particular spot, there are, at least four cautions to be attended to: (1) We must keep in mind the law that a current tends to move in straight lines, and hence, in winding channels, such as those under consideration, the advancing column must have impinged on the shore more at some places than at others. (2) Another very important factor to be taken into account is the extent to which the shore line at the spot was exposed to the prevailing high winds, and, in considering this point, it is necessary to remember, that, when great storms from the south-west happened at the time of the spring tides, the breakers would attack a higher shore line, and, for this reason, we may take it for granted that the sliding down of the shingle was intermittent, whereas the levelling and spreading out process by the action of the waves and tides was continuous. (3) We must take into account the extent of the sea-room to windward of the place, because, on it, depended the size of the breakers washing the shore; and (4) the position of the place with respect to the probable accumulation, or otherwise, of an unusual mass of snow and ice on the mountain right above it. This point is of some importance, because, although snow falls pretty evenly over a high range, yet, owing to the drifting action of the winds and partial thaws, it finally collects in the hollows and mountain valleys, and hence, on the advent of spring thaws, snow slips take place more frequently, and in greater force under such gathering grounds. Now on the Glen Roy "roads" the influence of the law must have been felt. A great mass of snow and ice could not have slid down at a time when the shingle along the shore line was deeply indented by the action of the sea, without carrying with it a much larger quantity than would have been the case had the shingle slid down alone. An obvious effect of such an avalanche would be to exhaust for a time the source of the shingle, and to explain why the "roads" are occasionally depressed below the mean, we have only to suppose that some such interregnum occurred when the high water mark was at, or near, the summit level of any of the cols. Another fact to be taken into account is, that, during the flood tide, high water occurs later and later at every step of its advance; and, in the case of a long narrow inlet, the tide keeps flowing at its head long after the ebb has set in at its mouth. For instance, high water is an hour earlier at Gravesend than at London Bridge. Similar causes produce similar effects; and accordingly we find that the lowest "road" is everywhere above the level of the lowest col. The obvious reason being that, after the high water level at the mouth of Glen Spean rose to the level of the Pass of Makoul, no interchange of the tides between the two watersheds could have taken place till the sea at the former point rose through a vertical space corresponding to the time occupied by the tide in travelling twenty miles through a shallow and narrow inlet; because, until the *first flood* passed through the col, no effect could have been produced in depressing the level in Glen Spean.

SEC. XI.—RETREAT OF THE GLACIAL SEA.

Doubtless these views have suggested to most of your minds the very pertinent question, If the Parallel Roads were formed during the subsidence of the land, how did they escape being effaced during its subsequent emergence? To this question, I think, I could give a satisfactory answer, supported by a great many facts and arguments, but unfortunately this paper has already exceeded all reasonable bounds, and I must therefore content myself with little more than a bare statement of opinion, which, I need hardly say, is never worth much. Well then, the retreat of the Glacial Sea from the North Atlantic basin was, as appears to me, a sudden event, and probably caused by the engulfment of the great Atlantic Continent, from which, in the Secondary era, flowed the river that deposited the sediment known as the Weald, and in the Tertiary era, the London clay and other tertiary deposits in the South of England. From the fossil, as well as from the botanical and natural history evidence, geologists have inferred that this ancient Atlantis included the Cape de Verd Islands in the south, and the Azores in the west; that is, it at least embraced an area of 2000 by 1200 miles. This region is now known as the "Great Glen" of the Atlantic, in which soundings frequently approach 3000 fathoms. The soundings, however, gradually decrease on both sides, and 2000 fathoms may be taken as a fair mean for the whole area. Now, on the supposition that this stupendous engulfment actually took place, we have a chasm of upwards of 5,000,000 cubic miles to be filled up, and, I need hardly say, this would set in motion the whole waters of the N. Atlantic: and this is not all. The removal of such an enormous mass of matter, as the engulfment presupposes, necessarily displaced the equator, removing it further south on our meridian, with the obvious effect of placing the ocean in both the N. Atlantic and S. Pacific above, and in the other two quarters of the sphere below, its natural level. This again destroyed the equilibrium, producing two great waves of translation, the one in the Atlantic travelling south, and the other in the Pacific travelling north, and we will not get a correct idea of the volume and initial force of the water thus set in motion unless we bear in mind what has already been urged with respect to the probable bulging out between the parallels of 41° and 51°. Because such great waves of translation, as I am suggesting, must have had their greatest development in mid latitudes, or rather in what were then the mid latitudes; and manifestly any bulging out necessarily added greatly to the intensity of the forward movement. In order, however, to interpret the phenomena we must not neglect the laws that determine the velocity and direction of a current. In addition to the fact that, on our meridian, the *vertical amount* by which the equilibrium of the ocean was destroyed, necessarily increased from the equator to probably about 65° lat., we must take into account the law that the velocity of water in motion increases with the depth; and we must also take into account that on moving from the pole to the equator the rotatory velocity of the earth increases, and this in our quarter of the sphere would give the current a westerly tendency, but on crossing the equator the corresponding tendency would be towards the east. There are grounds for suggesting that the ridge of the wave travelling up the Atlantic was from between ten to twenty degrees to the west of Ireland. From the much greater depth of water the centre or ridge of such a wave was necessarily far in advance of its two wings, and for this reason Southern Europe must have been overflowed from the north-west, and the east coast of N. America from the north-east. On the occurrence of the engulfment the water of course rushed in on all sides to fill up the great chasm of 5,000,000 cubic miles, and now known as the Great Glen of the Atlantic. Moreover the Caribbean Sea has been the scene of another great engulfment, and all the evidence goes to show that it also occurred at, or about, the same time. The engulfment, I am taking for granted, brought the north pole nearer Greenwich. This checked any movement across the equator to fill up the chasm or chasms; and it necessarily added immensely to the intensity of the movement from the north. Some idea of the extent of this movement may be gathered from the fact, that, at the time in question, the ocean retired from the British area to a depth approaching 3000 feet. The figure in the following page will perhaps aid us in seeing why the current moved southward in the Atlantic and northward in the Pacific.

Here *D B* represents the axis, and *A B C D* a great circle of the surface of the ocean at the beginning of the Tertiary era. From the causes already explained the axis shifted say, to *d b*. When this engulfment took place there was a sudden oscillation back, say, to the original *D B;* and this left the level of the ocean much too high in both the N. Atlantic and S. Pacific quarters. This, of course, caused two great Oceanic waves of translation, the one moving south in the Atlantic, and the other north in the Pacific. But columns of the Ocean, with a longitudinal extension nearly equal to a semicircle of the earth, once in motion, must have swept across both poles, carrying with them all the snow and ice that had accumulated during the previous ages. The question has probably suggested itself, Why did the protuberant body of water, marked in the figure as *W* and *X*, not spread themselves out laterally, as well as flow forward into the depressions *Y* and *Z?* The answer is, that there was doubtless a lateral movement; but there are two good reasons why it was uninfluential, as compared to the rush along the meridian lines: First, the shifting of the plain of the equator carried the protuberance along with it; and secondly, the meridians of about 100° W and 80° E respectively would be medial lines of little or no effect. Hence, in moving from north to south on our meridian, and from south to north in the Pacific, the waters simply obeyed the law of gravitation in rushing towards the greatest depression. I have already had occasion to remark that the sea retired from the British shores to a depth of about



3000 feet, and if we keep this in mind along with the fact that when the oscillation took place it left a protuberant body of water, extending from the pole to the equator, we can hardly fail to see how, on the one hand, the waters of the N. Atlantic were projected into the S. Atlantic; and on the other, those of the S. Pacific into the N. Pacific. Moreover, and as already suggested, the inertia of such great waves of translation, when once in motion, doubtless carried them across the poles, setting adrift all the snow and ice in both the Arctic and Antarctic circles, and, I need hardly say, like any other solid matter, an accumulation of snow and ice, enters as one of the factors in fixing the plane of mean momentum, and hence its abrupt removal must of itself have destroyed the equilibrium.

Let me now select a few facts out of hundreds that might be given, all going to prove that the drift sea produced an unusual but transient inundation. Between the Ural Mountains in the East, and the Rocky Mountains in the West, all the drift phenomena bear the strongest proofs of violence and universality. So much is this the case in northern Europe, that the late Sir Roderick Murchison attributed the great waves of translation required to explain it, to the upheaval of Scandinavia. But the same class of facts is everywhere seen on the opposite side of the Atlantic. On the northern side of the Labrador range, all the boulders are either swept clean away, or, where there is some barrier to the south, they are rammed together against the hill like a pavement. Passing to the southern side of the same range, the phenomena, as might have been expected, assume the form of great deposits of gravel. "These gravel hills," says Prof. Andrew, "are often sharp and conical and interspersed with deep circular valleys without outlets, from which the region has obtained the popular name of the Potash Kettles. . . . . . It would seem to be an unavoidable inference that our drift of this region not only came from the north, *but it came in a vast sweep of water deep enough to cover gravel hills 800 feet high*, and with velocity enough to throw such coarse material into lofty and steep summits." But the current that rammed the boulders together like a pavement against the northern face of the range, and piled up lofty hills of gravel under the shelter of its southern side, still moved onwards, depositing, in its course, what is known in America as the Orange sand formation, which is a superficial deposit, and excepting in places where it has been removed by subsequent erosion it covers all the inequalities of the

Southern States as "with a blanket," and finally disappears below the Gulf of Mexico at about 450 feet below the present level of the sea.

"Such being the general disposition of these deposits," says Dr Hilyard, State Geologist of Mississippi, "we are led to inquire into the origin of the great inundation of water, apparently devoid of organic life, by which they were formed. What traces of its existence has it left north of the latitude of the Ohio, whence the rush of waters evidently came? The northern geologist may pertinently retort, what became of that rush of waters, whereof our drift furnishes the evidence, after it left our latitudes?"

But the drift phenomena only disappeared under the Gulf of Mexico to re-appear in S. America. For we know, from the labours of Agassiz, that the clays or earths characteristic of the period are everywhere present in Brazil. Even at 4000 feet above the level of the sea, the deposit covers all the undulations of the country, and where there are caves it enters them, sealing up the remains of the extinct mammalia. Further south still, where the easterly trend of the Cordilleras, and the mountains of Patagonia, are in a position to have arrested the onward flow of such a current, we have the great deposit of calcareous earths known as the Pampas; and it is of moment to remember that the mammalian remains found *in* this great formation belong to the same extinct Fauna as those found *under* the drift in the bone caves of Brazil. It is true Dr Darwin considers the Pampas to be merely an ancient estuary of the Rio de la Plata, but to this there are numerous objections; however I must content myself by stating only one, namely, the depression of land which Darwin's view presupposes would have annihilated the river in question, and at the same time have converted Brazil into an island.

Let us now turn to the eastern hemisphere where we meet with exactly the same series of facts, but beginning at the opposite pole. In both Australia and New Zealand the drift in some form is present everywhere, and the latter country being nearer the meridian of maximum effect, the phenomena furnish evidence of greater violence than in the former. But the Australian drift is highly interesting in this respect, that, at all levels up to at least 1000 feet, it is shown to belong to one and the same period, because, like warp in a web, the different portions are all woven together by the remains of the same species of mammalia. Now if we follow the track of the wave to the north, instead of the coarse gravels of New Zealand, we find, in Eastern Asia, a great development of clayey earths with calcareous concretions of exactly the same description as those in Patagonia. In China these earths puzzle the geologist under the name of the "Chinese Loess," and appear to be in great force wherever the mountains to the north and north-west confront what must have been the direction of the current. In explanation why a north-west range stopped the force of the current, we must keep in mind that such a great wave of translation, as these views imply, must have crossed the equator with a breadth nearly equal to the semicircle. On travelling north, however, the breadth tended to contract at the same rate as the degrees of longitude, and this tendency raised the wave vertically, and any vertical increase necessarily generated lateral movements on both sides. In accordance with this view, the barrier ranges surrounding China and Thibet must have formed a *cul de sac* to the westerly wing of the great wave. "The alluvium of *West Nari*," says Capt. Strachey, "in what may be presumed to be its original completeness, appears to attain its maximum elevation about 16,200 feet; at 16,500 it may be seen just rounding off the mountain feet and connecting them by undulating slopes with the lower flats. . . . . Limestone seems to impart a great consistency to the alluvium, and deposits of this material sometimes occur which are *aggregated into a solid rock, too hard to be broken by the hand.* At Guge fossil remains of large mammalia are found in the formation, and at the same place the great ravines are flanked by cliffs *half a mile high*, which *exhibit throughout their thickness a fine homogeneous clay* with little gravel in it."

"The immense extent," continues Captain Strachey, "of the existing alluvium, and the uniformity of its maximum elevation, lead me to infer that it must have been deposited under a general sea covering the whole country (in which it so occurs), and not by lakes, much less by rivers. The existing lakes are comparatively mere puddles, made by the drainage of the higher and smaller mountain ravines stagnating in slight hollows of the old alluvium, and the gigantic ravines in the ancient plateaux are out of all proportion to the contemptible rivulets that now creep along their bottoms, wandering over beds a thousand times too large for them."

Captain Strachey, it will be observed, concludes that the alluvium of Thibet was deposited at a time when the sea covered the whole area; but if, of marine origin, there are obvious circumstances which prove that this could not have been under ordinary marine conditions. A submergence to a depth of 16,500 feet would convert the whole area into an archipelago, and in that case where were the rivers to deposit the fine homogeneous clay to a depth of half a mile? Also, where were the pasture grounds of the large mammalia, whose fossil remains are now embedded in the formation, and what has become of the co-temporary deep sea deposits over the plains of India? Moreover, how are we to account for the presence, at a lower level of 10,000 feet, of the freshwater formation of the Sewalik Hills, with its remains of numerous species of mammalia, including such animals as monkeys, hyenas, elephants, mastodons, rhinoceroses, horses, hippopotami, camels, giraffes, oxen, sheep, &c. In short, the view that the sea stood at the necessary elevation, and for a sufficiently long time to deposit, under ordinary conditions, such an extensive formation, is quite untenable, and the usual scapegoat of the geologist, "give me sufficient time," cannot afford much aid in reconciling or explaining such facts. We have seen that these clays, present in such force on the elevated plateau of Thibet, contain mammalian bones, and in connection with this it is interesting to find a corresponding formation also containing similar remains in the elevated valley of Quito in the Andes.

"At Alangasi, six miles from Quito," says Mr James Orton, in the *American Journal*, "teeth of the Mastodon have been found, and in the ravine of Chalan, seven miles south of Riobamba, we discovered the femur and patellae of a Mastodon, the skull of a horse, and numerous leg bones not yet identified. They were embedded in the middle of a cliff, 200 feet high, of the compact tenacious clay resulting from the union of trachytic ashes with water, and were associated *with terrestrial shells* identical with living species in the vicinity. The bones were drifted to this spot and deposited (many of them in a broken state) in horizontal lines." Quito, as is well known, is surrounded by the largest volcanic mountains in the world, and the author cited above tells us that the great valley is now covered by a vast layer of pumice and scorae, which have *been falling from the early tertiary to the present time*, and yet into this elevated valley 10,000 feet above the sea, the pleistocene clay has been intercalated; and, as if to make sure of its identity, the drifted and broken mammalian bones are placed in the middle of the cliff. But let us follow the track of the great Pacific wave to the north.

In Siberia we find the same clayey earths spread over the flat Tundras to such an extent that a person may travel thousands of miles without seeing a stone. Again, in the Icy Sea, there are islands some two or three hundred feet high, which are said to be literally made up of drift wood and animal remains, all encased in a matrix of the same clayey earth. Speaking of the trees in those islands, Erman thus writes: "On the summit of the hills they lie flung upon one another in the wildest disorder, forced upright in spite of gravitation, and with their tops broken off or crushed as if they had been *thrown with great violence from the south*, and there heaped up. To suppose that these islands of half-fossilized wood and bones are merely the annual accumulations of river drifts would not be a credible supposition, and for the simple reason, that the depth of sea necessary to carry them to their present situation would inundate all the flat swamps known as the Tundras of Northern Siberia, thereby throwing back the mouths of all the rivers at least 700 miles from the spot. Another consideration which must be kept in mind is that all the wood and bones in question are almost restricted to the yellow clay, being but rarely found in the alluvium which overlies it, and never in sand. Moreover, the further north this clay can be reached the wood and bones increase in frequency; and we will fail to see the importance of the facts unless we bear in mind that the deposit in some form or other stretches from the base of the Ural Mountains, where, from the trending of the range, it is in great force, right across Siberia, and passing Behring's Straits it reappears in North-west America; and throughout the whole of this semicircle it not only contains mammalian bones, but bones of the same age and the same species; and, not only so, the formation itself everywhere possesses the same properties, and is essentially of the same character. I say essentially, because there is one particular in which it varies. The deposit in Siberia appears to contain less lime than further south, but there is nothing in this but what ought to have been expected, for, manifestly, such a wave, as we are contemplating, must have reached the southern shores of Eastern Asia turgid with the calcareous ooze of the Pacific, and in such circumstances we may take it for granted that the waters parted with more of the lime in China and Thibet than during the later stage of their overland journey. It has already been pointed out that the wave, in passing the equator, had a breadth nearly equal to the semicircle, and hence, in travelling north, it had either to rise vertically or spread laterally. In the case of the Atlantic wave, the latter tendency, combined with the easterly trending of the Cordilleras has, as already remarked, caused the deposition of the Pampas of La Plata, and we find that the same causes have produced the same effects in Siberia. After passing 64° N. lat., the Ural chain, under the designation of the Obdorsk Mountains, trends to the north-east, and the consequence is, the diluvial clays are in great force in front of this bend in the range. But from the nature of the case the law must be universal, for it is manifest that wherever a high range crossed the path of the wave, or where from the contour of the surrounding mountains the wave entered a sort of *cul de sac*, all the waters below the level of the ridge of the obstructing range would come to comparative rest. But as water in motion carries matter in suspension, in proportion to the force of the current, an arrest could not in this way be placed on the velocity of the current without giving rise to a voluminous deposition of sediment, and the deposition would proceed so long as the turgid waters passed overhead. Of this we have a good illustration in Europe. The chain of the Alps extends in a north-east direction through the Tyrol to near Vienna, and the valley of the Rhine is again enclosed on the east by the mountains of the Black Forest, and on the west by the Vosges. In these circumstances the wave travelling over the continent of Europe from the north must have here entered just such a *cul de sac* as I have been describing, and, as a natural consequence, we have the extensive sedimentary formation well known as the Loess of the Rhine. The late Sir Charles Lyell attributed the Loess to the deposition of glacial mud; but, as appears to me, there are several reasons which go to show that such a view is untenable. However, obvious considerations with respect to time and space prevent me from adverting to more than three of them. First, the Loess contains 20 per cent. of lime, which according to Bischoff, must have been deposited in a state of mechanical suspension, but the Rhine, after passing the Lake of Constance, carries no lime in suspension. Secondly, the hypothesis requires a sufficient depression of the central region to convert a large portion of the watershed into a fresh water lake, or in other words, that the Rhine was damned back till it made large deposits of sediment 800 feet above its present level. But it can hardly be necessary to remark that in such circumstances we ought to find the most ample evidences of an inland sea. Every stream flowing into a lake forms a delta, and hence, on the retreat of the waters of the hypothetical lake, all the tributary streams must have cut their channels through their own deltas; but instead of this having been the case, all the streams in the lateral valleys appear to have re-excavated their courses through homogeneous loess. Thirdly, a vertical depression of 800 feet at Fribourgh implies that the lake or inland sea was of great longitudinal dimensions. But a great river could not have entered a lake of this description at one end and passed out at the other without leaving abundant proofs of the assorting power of water. For instance, the waters of the Rhone, on entering the Lake of Geneva, are turbid and discoloured, but on issuing from the lake at the lower end they are beautifully clear and transparent, and this is manifestly inconsistent with the homogeneousness of such an extensive deposit as the loess.

In conclusion, permit me to make just one or two allusions suggestive of a line of argument in support of the suddenness of the retreat of the glacial sea—a line of argument that in my opinion can be amplified to almost any extent. High on the Scottish and other European mountains, the common seapink, the scurvy grass, and the Plantago maritima are all found growing among the rocks, some of them in great profusion, and except where they have been brought down by mountain streams they are found nowhere else but along the present sea shores. Now for these horizontal and parallel zones in the vegetable kingdom there must be some adequate cause. As regards the lower one, we know that the plants in question represent sea-shore conditions, and this being so, at what other conclusion can we arrive than that the other also marked off the position of an ancient coastline? But such a conclusion seems to require us to believe that in a comparatively short time the Continent of Europe rose vertically at least 2000 feet; because, on any other supposition, it is impossible to account for the wide intervening zone in which the plants in question are unrepresented. It will not do to answer that over the intermediate blank the conditions are unfavourable, for the exceptional instances in which the plants have been carried down by the mountain streams prove the contrary.

In England and other countries, the ancient tertiary soil, still containing stems and roots of trees, is covered up by the drift gravels, and this suggests the pertinent question, was it possible that this ancient soil could have survived the shore-action of the breakers during both a gradual depression and a gradual emergence of the land? Again the drift gravels contain numerous pieces of chalk and other stones whose fractured edges are perfectly sharp and unworn, and what other explanation can be advanced to account for such facts except that they were broken by violent movements and suddenly covered up? But what better proof of the suddenness of the drift phenomena can be adduced than the drift itself? In all lands, and at all levels, it occupies exactly the same position with respect to the other formations. Wherever it is present in sufficient force to be recognised it forms a roof either to the Tertiary or older strata, and in not one instance does the Pliocene or late Tertiary form a roof to it. Again it everywhere bears the same relation to the extinct mammalia; nearly four hundred species can be traced up to the edge of this thin boundary-line in time, but they, there and then, cease to belong to the list of living creatures. Nay more, did time permit, it might be shown how impossible it is, without such an inundation as I am suggesting, to account for the present distribution of *the living terrestrial animals* over the sphere. Further, the surface deposits are everywhere suggestive; here, where from the contour of the land two currents necessarily met, mounds of gravel are thrown up; there, where a country was overflowed from opposite sides, the meeting of the two waves is indelibly marked off by lines of kames or eskers; in other places, all the earth and boulders have been swept into ravines and other sheltered places; every natural basin surrounded by ridges has received a bed of clay or brick earth, and gorges, having a north and south direction through mountain ranges, are swept clean of everything moveable, whilst the debris of the movement is found collected in protected situations. But perhaps the strongest piece of evidence is the fact that the drift in one form or another is found at all levels, and the sea has retired without leaving one solitary trace of a sea-beach or evidence of sea-shore action. In America thousands of miles of railways over virgin plains have been made in all directions, and a beach of sand or of gravel would, we are told, have been a godsend to the engineers, but not a trace or vestige of such a thing ever rewarded the most careful search.

Just a concluding word. I may reasonably be asked if the retreat of the Glacial Sea was with the suddenness and violence which these views imply, how did the Parallel Roads escape being effaced? The answer is to be found in the height and contour of the surrounding mountains. On both the north and south sides, Glen Roy is com-[illegible]y sheltered, and hence when the southward rush took place the current must have passed harmlessly overhead, the "roads" all the while remaining safely enveloped in deep and comparatively still water.*

SEC. XII.—THE COSMICAL CHANGES OF LEVEL DURING THE EOCENE AND MIOCENE PERIODS?

For sake of clearness I have put in a tabular form the direction of each succeeding oscillation since the beginning of the Tertiary era; but the division of the sphere shown on fig. V. is to be understood as only applying to the one referred to in the heading of this section. Another caution may not be unnecessary. In cases of a depression of the land we are not to expect to find direct proofs of the fact except over areas where a subsequent elevation has brought the depressed zone once more above the level of the sea. Again, it is important to keep in mind that the N. Atlantic quarter of the sphere is the only one with respect to which much information has been accumulated, more especially as to the smaller changes of level.

NORTH ATLANTIC.	SOUTH ATLANTIC.	SOUTH PACIFIC.	NORTH PACIFIC.
a. Gradual Depression.	*b.* Gradual Elevation.	*c.* Gradual Depression.	*d.* Gradual Elevation.

NATURE OF THE PROOF.

a. Excepting areas of local disturbance, all the Eocene marine formations round the North Atlantic basin are below the level of the sea; whereas the newer formations are at considerable elevations above it, proving a depression of the land. It has been shown, I think by Professor Prestwich, that the London clay has been deposited to a depth of 400 feet, the estuary all the while remaining shallow. This proves a gradual depression.

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* This concludes the original paper, and what follows has been added in the hope of strengthening the argument.

b. Round the coasts, both of South Africa and South America, there are early marine Tertiary formations above the level of the sea, but there is a complete gap between these early Tertiary strata, in which all the species are extinct, and the superficial Pleistocene deposits. Darwin describes beds on the Parana: "This vertical section clearly tells us of a large bay of clear salt water, gradually encroached on, and at last converted into the bed of a muddy estuary into which floating carcases were swept." The importance of this evidence of a gradual upheaval of the land will be seen when the following statement by the same author is taken into account. Sedimentary matter containing fossil remains belonging to the ancient Tertiary formations "has been deposited and preserved at different points in north and south lines, over a space of 1100 miles, on the shores of the Pacific, and of at least 1350 miles on the shores of the Atlantic, and in an east and west line of 700 miles across the widest part of the continent." (Of South America.)

c. So far as my knowledge goes, no beds proved to be Eocene have been found in either Australia or New Zealand, but the nearer the geologist goes to the coast-line he finds the lower Tertiary formations to become older. In a paper on the geology of Queensland, Mr Daintree collects information with respect to a superficial deposit, which he calls the Desert Sandstone, and concludes that, "without doubt it is the *most recent* widely spread stratified deposit developed in Queensland," and adds that all the available evidence tends to shew that it nearly covers Australia. Again, as regards New Zealand, Dr Hector has concluded that the islands were submerged to a depth of 1800 feet.

d. Whilst on the one hand the Atlantic border of North America was an area of submergence, that portion of the continent comprising the Gulf States was, on the other, one of elevation; large additions to the dry land having been made during the period. Again, there are Nummulitic formations round the coasts of China, and also in the Phillipines, and, according to Mr Pumpelley, no marine Tertiary formations older than the China Loess have been found within the limits of China proper. Independently of this negative evidence, the similarity of the terrestrial mammals of Asia and North America proves that during the Tertiary era, dry land communication must have existed between the two continents: for it is out of the question suppose that such animals as hyenas, dogs, panthers, mastodons, horses, camels, elephants, rhinoceroses, &c., could have crossed by means of drift ice. But dry land over what is now Behring's Straits would require an elevation of at least 300 feet.

ADDITIONAL REMARKS.

Taking it for granted that the above and following oscillations were caused by a shifting of the axis, a suggestion or two may be thrown out as to the amount of this first displacement. From his study of the fossil flora, Professor O. Heer has concluded that, in the Miocene period, North Greenland had a climate at least 30° Fahrenheit warmer than the present one; and his conclusions with respect to Greenland are strikingly confirmed by the flora of Spitzbergen, where, in 78° N. Latitude have been discovered "the beech, plane, hazel-nut, and some other species identical with those from Greenland." From the fossil plants in the isle of Sheppey, Bowerbank has identified thirteen species of palms, proving a much warmer summer than now. Again, according to Ettingshausen, the Tertiary flora of the Vienna basin corresponds to a temperature of between 68° and 79° F. But Vienna is now on the isothermal line of 50°; and if, at the present day, we want a climate to correspond with its Tertiary flora, we must go 20° further south, say, to about Morroco or Egypt. With respect to this evidence there are, however, three sets of circumstances to be taken into account: first, the climate of the Arctic circle was doubtless modified by the re-distribution of the waters of the ocean, that took place at the beginning of the era; secondly, with respect to the plants in the London Clay, we must remember that the river which transported them to the sea flowed from the south, and was of unknown length and volume; and thirdly, Europe, at the time, must have been converted into an Archipelago, with a warm current flowing from the Indian Ocean over what is now Egypt, into the Mediterranean; and this could not have been uninfluential in raising the mean temperature at Vienna. Taking these circumstances into account, I think a shifting of the equator on the meridian of Greenwich of from 10° to 15° north of its present position, would probably satisfy all the conditions which the flora present.

We have seen that during the middle and late Tertiary Era both New Zealand and Australia were more under water than now; and as regards climate, Mr. Jenkins, in a discussion at a meeting of the

Geological Society of London, stated that he had, from a study of the mollusca, arrived at the conclusion that the Tertiary beds of Australia were deposited under a warmer climate than at present. This is significant; because, with a smaller land area, the climate ought to have been colder, as manifestly, the great extent of the existing arid plains must tend to raise the temperature. And so indeed, they do, for, on approaching the land, all the isothermal lines on both sides of the continent are deflected towards the south, proving conclusively that the dry land exercises a heating influence. In a word, unless we assume a shifting of the axis, it is difficult to conceive conditions under which the temperature could be raised. With its plains singularly exposed to the solar influence, and its shores bathed with the equatorial current, and, at the same time, lying in the track of the warm breath of the south-east trade winds, we naturally expect to find Australia in the enjoyment of its maximum temperature. And when, considering the existing conditions, we must not forget that when the mollusca, from which Mr. Jenkins drew his conclusions, lived in the Australian seas, a great part of that continent was under water, and the effect of this must have been to depress the temperature. Hence when, in the face of such facts and circumstances, we find the temperature in Tertiary times higher than at the present day, the onus to account for such disagreements between cause and effect, clearly rests with those who maintain the permanency of the axis.

SEC. XIII.—THE COSMICAL CHANGE OF LEVEL DURING THE LATE TERTIARY PERIOD (PLIOCENE?)

NORTH ATLANTIC.	SOUTH ATLANTIC.	SOUTH PACIFIC.	NORTH PACIFIC.
a. Unimportant Elevation.	*b.*	*c.*	*d.*

a. On the coast of Norfolk the Cromer forest bed overlies the Norwich Crag formation, proving that dry land conditions followed *marine* Pliocene deposits; and if we go west to Devonshire we find the same history in Kent's Hole. To explain the phenomena of this celebrated cavern, we must contemplate Warberry Hill far enough above the sea-level to be covered every winter with snow; and for the snow and ice to be occasionally present in sufficient force to divert some of the streams into the cavern. This produced the breccia. But the sea-level was rising, and bringing in milder conditions; and the milder conditions covered Warberry Hill with vegetation. The decay of the vegetable matter generated carbonic acid gas; and this gas carried down by the rain water through the rocks forming the hill dissolved the limestone. At the present day, owing to the closing up of the old entrances, the temperature inside the cavern does not vary from the one end of the year to the other, and hence, the deposition of stalagmite is all but impossible; but at the time I am speaking of, when all the entrances were open, with greater extremes both in the heat of summer and the cold of winter, the meteorological changes inside must have moved in sympathy with those outside, and this again must have maintained an active circulation of the air. The circulation necessarily promoted evaporation, and the result was the deposition of the crystalline stalagmite. The land, however, kept on sinking, and the sea at last invaded the cavern. This is proved by the breaking up of the stalagmite floor; but as the floor is disturbed only in the inner reaches where the violence of waves becomes concentrated, we can, from this fact, infer that marine conditions did not last long. The cave-earth which follows the crystalline stalagmite proves an elevation of the land, and according to my reading of the phenomena, this elevation synchronizes with that in Norfolk and Suffolk. The same upheaval is proved by the superposition of the Tertiary deposits in the Mayence basin. After forming "banks of oysters, covered with parasitical shells, corals, sea-acorns, heaped bones of the sea cow, and numberless sharks' teeth," the sea, according to Dr. Sandberger, retired. "As gradually as the land sank it rose again, passing through all the stages from strongly saline brackish water to a fresh water lake."

b. c. and *d.* I have no information with respect to these quarters of the sphere; but according to my view the small elevation in Europe was a part of a cosmical change. The previous great oscillation was necessarily arrested so soon as the mass of land raised above the level of the sea in the S. Atlantic and N. Pacific balanced the masses of ice and water accumulated in the other two quarters of the sphere. But notwithstanding this arrest, the amelioration of climate continued with a gradual rise of the temperature of the ocean, this again caused the retreat of the snow and ice, with the effect of reversing the oscillation. We will find an exactly similar reversal when we come to the great Post-Tertiary oscillation.

SEC. XIV.—THE SUDDEN COSMICAL CHANGE OF LEVEL AT THE DRIFT ERA.

NORTH ATLANTIC.	SOUTH ATLANTIC.	SOUTH PACIFIC.	NORTH PACIFIC.
a. Sudden Elevation.	*b.* Sudden Depression.	*c.* Sudden Elevation.	*d.* Sudden Depression.

a. From its very universality it is difficult to state the proof of the suddenness of the elevation. The vertical amount over the British area was probably about 3000 feet minus the extent of the small gradual elevation adverted to above; that is, from something approaching 2000 feet above its present level, the sea sank to about 1000 feet below it. It abandoned the whole of the intervening zone of dry land without leaving a trace of a sea-beach or of littoral conditions. Wherever, on the surface, there is a natural basin, there is an evenly spread out deposit of clay or brick earth, but no evidence of the action of waves and tides. Between what is called the glacial deposits and the Clyde beds there is a gap in the continuity of mollusks, which the Scotch geologists have not been able to fill up. Such a gap is inconsistent with a gradual elevation of the land, but it is in strict accordance with what I am suggesting. Because, if the sea came to rest at, say 1000 feet below its present level, the mollusks would have ample opportunity during the subsequent depression of the land to re-adjust their respective habitats; and, of course, all the evidence of the change is now hundreds of feet below the sea. It will be remembered that in my paper I have called attention to a similar gap among the plants. Certain sea-shore species still thriving along the high-water mark are also found on the Scottish mountains, about 2000 feet above the level of the sea; but except in rare instances, in which they have been carried down by mountain streams, they are altogether unrepresented over the intermediate zone; and this of itself affords strong evidence of a sudden elevation. The rivers and streams of Scotland have cut their way through the "till" and drift deposits generally; and whilst that was going on every river must have deposited extensive deltas at its mouth; and, with a gradually rising land, again cut its channel through its own delta; but there is not a trace of any such delta, and the beds deposited during the subsequent depression of the land rest directly on the old river channels cut through the Boulder Clay. Around the coasts on the eastern border of the North Atlantic peat-bogs, buried forests, and old land surfaces are known to exist at many different places below the low-water mark; and as the land has risen some thirty or forty feet since the arrival of the Celtic race, it is clear that these proofs of a previous submergence were at one time much deeper under the sea. Again, Mr. J. Gwyn Jeffreys, when dredging off the Shetland Islands, brought up numerous specimens of fossil Arctic shells from a depth of ninety fathoms. They were littoral species, proving that in Post-Tertiary times the low-water mark was at least 600 feet lower than now. It has been inferred from the fauna that there was actual dry land between Britain and the Continent; and Mr. Austen (R.A.C.) has concluded that sub-ærial beds resting on strata of the age of the Crag, charged with angular stones, are due to the disintegrating influence of severe frosts, *when the land stood much higher out of the sea than at the present day.* Crossing the Atlantic, Hillyard tells us that the Orange Sand, which is the southern continuation of the Drift of North America, is, in the Gulf of Mexico, about 400 feet below the level of the sea; proving that the land stood higher at the close of the drift period than now. Again, off Nantucket, the dredge has brought up gravel from a depth quite beyond the action of the present shore line; and by careful soundings the channel of the Hudson has been traced for some distance out to sea: all going to prove that the land then stood higher out of the sea; and Dana has concluded that this elevation increased in going north.

b. "I have convincing proofs," says Darwin, speaking of the northern parts of Chili, "that this part of the continent of S. America has been elevated near the coast at least from 400 to 500 feet, and in some parts from 1000 to 1300 feet since the epoch of existing shells, and further inland, the rise possibly may have been greater. * * * * * The old underlying Tertiary formation at Coquimbo appears to be about the same age, as several deposits on the coast of Chili (of which that at Navedad is the principal one) and as the great formation of Patagonia. Both at Navedad and in Patagonia there is evidence that since the shells there entombed were living there has been a subsidence of several hundred feet, as well as an ensuing elevation." * * * * * "Everything on this southern continent has been effected on a grand scale; the land from the Rio Plata to Tierra del Fuego, a distance of twelve hundred miles, has been raised in mass (and in Patagonia to a height of between 300 and 400 feet) within the period of existing sea shells." * * * * * "The elevatory movement and the eating-back power of the sea during the periods of rest have been equable over lines of coast; for I was astonished to find that the step-like plains stand at nearly corresponding heights at far distant points."

Now, crossing to the eastern border of the South Atlantic, we find precisely the same conditions of level. Mr. Stowe has described Post-Tertiary formations which cap the hills round Port Elizabeth, in Cape Colony; and as there are raised beaches between this formation and the sea level, it is clear that the sea has retired, as we have just seen was the case in S. America, and the same thing holds good with respect to the Mauritius. "Great masses of coral," says Mr. George Clarke, "some of forty or fifty tons weight, are seen on various spots on the coast of Grand Port, at a distance of a quarter of a mile from the sea, and at least fifty feet above its level. These are of a species not now found here in a living state." Mr. Stowe also states that the evidence of what he calls the Pliocene or Post-Tertiary shells of the superficial limestone, of the Zwartkops heights and elsewhere, leads us to believe that *the climate of S. Africa must have been of a far more tropical character than at present.* And during the discussion that followed the reading of Mr. Stowe's paper, Mr. Searles Wood, Jr., "observed that there appeared some probability on the face of the paper, of the shells of the older Post-Tertiary beds, *denoting a warmer climate than the present, instead of, as here, a colder.*" Now, it can hardly be necessary to observe that any shifting of the axis further south on the meridian of Greenwich, which raised the British Isles out of the sea, would depress S. America and S. Africa; and at the same time, cause just such an interchange of climatal conditions as is indicated above.

c. Dr. Haast has obtained ample evidence that in early Post-Tertiary times the glaciers of New Zealand descended to much lower levels than they do at the present day. "Although the size of the principal glaciers is enormous considering the narrowness of the central chain, the astonishment of the geological observer increases on meeting everywhere the signs of a still more considerable glaciation during the Pleistocene period." Taking into account the isolated position of the islands it would not be warrantable to attribute this excessive development of glaciers to any possible arrangement of the aerial or oceanic currents; and this compels us to conclude that New Zealand stood higher out of the sea. The same conclusion may be come to with respect to Australia, because, from the fauna and flora, Mr. A. Wallace has adduced good grounds for believing that, in comparatively recent times, dry land communication existed between that continent and some of the Islands of the Eastern Archipelago; and this is just one of the effects that must have accompanied an elevation of that quarter of the sphere. With respect to the suddenness of the elevation I have no direct evidence; but it may be inferred from the fact that, as is the case in Europe, the Drift is a universal phenomenon, and excepting a coast margin, it is nowhere, in Australia and New Zealand, modified by the shore action of the breakers. At any rate, in my reading (limited it is true) on the geology of those islands, I have not met with a single notice of a sea-beach or a marine deposit subsequent to the Drift.

d. The Bay of Bengal is surrounded by a fringe of a deposit known as the Coast-laterite. "The position occupied by the laterite is," says Mr. Foot, "that of a belt running parallel with the general coast line, but broken through, at many points, by the rivers falling into the Bay of Bengal." * * * * * "That the low-level gravels and conglomerates of the coast-laterite and the coarse boulder gravels of the middle grounds, including the great accumulations of the quartzite shingle, were deposited or re-arranged into their present form during one and the same period, and are marine deposits" * * * * * "That during the latter part of this period an elevatory movement was in progress, *by which the land was raised* between 500 and 600 feet. This was followed by a period of quiescence, during which the laterite deposits underwent very extensive denudation, particularly from the action of the rivers, which cut passages for themselves across the belt of recently raised land, and in great measure scoured out the laterite deposits from the old river valleys, which had been scooped out of the gneissic rocks, probably in much more ancien times." Here we have proof that the belt of drift, converted into laterite, was either suddenly covered by the sea, or that the sea came to rest 500 or 600 feet above its present level.

Another point worthy of notice is that the ancient river channels were filled with the Drift, and were not re-excavated till after the subsequent upheaval. Speaking of a stratified formation in the island of Yesso, Japan, in which the fossils were "scarcely distinguishable from the Mollusca now existing upon the coasts," Mr. W. P. Blake concludes: "In these deposits, and in later terrace-like formations, there is abundant evidence of the comparatively recent uplifting of the whole island, and the *same evidences* are found upon the island of Nipon." Again, Mr. Logan has furnished proof of an elevation in the Malay peninsula. He says, "that most of the hills scattered along the western plains of the peninsula were islands in the sea at no remote period."

In China proper, the older strata are concealed by a profuse alluvium, and I know of no evidence on the one side or the other; but on going north, proofs of a depression are abundant. In Siberia, marine shells of living species are found 500 miles south of the Arctic Sea, and Russian geologists have concluded that the present severe conditions of climate are attributable to the subsequent elevation of the land.

Thus we have indications that the Drift was followed by a simultaneous elevation over the North Atlantic and South Pacific, with a second glacial period in each of these two quarters of the sphere; whilst, in the reciprocal quarters, there was the corresponding depression, with an amelioration of climate.

SEC. XV.—THE DRIFT OSCILLATION DESTRUCTIVE TO THE TERRESTRIAL MAMMALS.

Those who dispute that the great oscillation of level, discussed above, was sudden; and that its suddenness produced great waves of translation, have many serious difficulties to get over. One thing is certain, the glacial sea could not have retreated from the North Atlantic basin with the suddenness indicated at the close of section XI., without proving destructive to the terrestrial mammals. "In South

America," says Dana, "over one hundred species of *extinct* Post-Tertiary quadrupeds have been made out. The bones occur in great numbers over the prairies or pampas of La Plata, and in the caverns of Brazil; and these include some thirty species of Rodents, species of Horse, Tapir, Llama, Stag, a Mastodon (different from the N. American), Wolves, and half a dozen Panther-like beasts, which occupied the caverns of Brazil, Ant-eaters, twelve or fourteen species related in tribe to the Megatherium (Sloth-tribe) and a dozen or more related to the Armadillo. *They number more species than now exist in that part of the continent, and far larger species.*" Now it is of great importance to bear in mind that the remains of these extinct mammals are *in the Pampas Clay*, but *nowhere* above it, and that they are also *in the Brazil Caverns*, but *there the clay simply covers them up.* In N. America the destruction of terrestrial mammals was even greater; the extinct mammalia included an Elephant; a Mastodon, larger than the European one; "Horses," says Dana, "much larger than the moderns"; species of Ox, Bison, Tapir, gigantic Beavers, species of Dicotyles (related to the Hog), also animals of the Sloth tribe, of the genus Megatherium, Mylodon, and Megalonyx, of great size, compared with those now living.

"Among Carnivores there were a Bear, a Lion, and a Racoon; * * * * * There were also a stag, whose horns exceeded in size the Irish Elk, a Rhinoceros, an animal allied to the Hippopotamus, and a species allied to the Camel. In other words, the Tertiary fauna of N. America differed very little from that of the old world." Now, by what fell stroke did all these animals disappear? They are under the drift; they are in the drift; but they are in no formation overlying it. Manifestly in the case of an inundation of the land by the sea, arboreal animals and animals that were good swimmers would find in floating forests and rafts of drift wood the means of escape, at least in sufficient numbers to propagate their kind. Keeping this in view, the living animals of S. America are nearly all arboreal, even frogs living on the trees; and when, with respect to the northern half of the continent, we leave out some twenty or thirty quadrupeds common to the whole Arctic circle, a dozen or so that are strictly arboreal, and a few aquatic species, the mammals of the two Americas are nearly exhausted. True, it is not very easy to see by what means some of the living animals contrived to escape; but any difficulties felt on this ground will, I hope, disappear when we come to realise what must have been the necessary effects of such great waves of translation upon the forests of the world. In our own latitudes hardly a season passes without numerous instances in which not only individual patriarchs yield to the storm, but whole clumps of trees are laid prostrate in an hour; and if the wind is capable of effecting all this, it surely may be taken for granted that water, which is 810 times the weight of wind, would sweep a forest before it like so many feathers. For instance, in 1818, Mr. Buchanan of Ardoch witnessed a debacle in the Valley De Bagne. "The torrent, with all the debris" (the water could scarcely be seen) "carried along with it, rushed down the valley with such force that large trees were swept away like twigs, *falling towards, not in, the direction of the stream:* that is to say, that the impetus was so great that the ground was cut away from beneath their roots, so that they were not solely borne down by the force of the current." This occurred in Switzerland, and it will help our imagination to picture to ourselves what would be the effect of a great wave of translation sweeping across the primeval forests of America. General Miller, speaking of what is called a road in Eastern Peru, says: "At places the track is so completely roofed over by timber trees *that have fallen across* that it becomes a dark subterranean-like passage." "The forest," says Schomburgh, speaking of Guayana, "here reigns triumphant; all traces of civilisation are left far behind, above, around one dense mass of foliage." * * * * "The wild vine is seen at times twisted like a corkscrew round the loftiest trees; at others intertwined like the strains of a cable, then drooping on the ground and again taking root, and thus as it were securely anchoring the trees against the fury of the sweeping blasts." After stating that in tropical forests individual trees are larger than in the temperate zone, Professor Schouw tells us: "Climbing plants, themselves arborescent, twine round the trunks, and frequently acquire such a mastery as to choke them, and at length wind round *bare dead cylinders.* They often seem to have compressed the trunk like snakes, which arises from the bark and wood having grown up over these climbers: frequently they hang like garlands from one trunk to another, or they twine to a length of several hundred feet, like a cord, from tree to tree: and since these trees stand very close together, it is only possible to make a way through such a forest by hewing a path."

These extracts will give us some idea of the huge islands of entangled forests that would get adrift in such an inundation as I have suggested. And it can require no great draft on our imagination to see how, on such aggregated masses, the smaller quadrupeds may have found a foothold and refuge from the impending danger. True, and as already remarked, the means by which numbers must have escaped are not apparent; but what could have been antecedently more improbable than that sheep, during a local inundation, could have contrived to escape by scrambling into the branches of trees: and yet we have the authority of Sir Thomas Dick Lauder that such was the case. In his Morayshire Floods he tells us: "One sheep was seen to cross the Spey to the south side on the 4th (August, 1829), and on the 5th it was again feeding about the house, on the north bank; several others *were found alive on the tops of the trees* at the foot of the garden, having scrambled into them when they were bent down by the pressure of the water." Of four pigs, only six months' old, belonging to the miller of Garmouth, one swam four miles east and landed at Port Gordon, and the other three swam five miles to the west, and landed at Blackhill. "Where there were no trees, rabbits were drowned in great numbers, but where there were plantations, and especially where there were spruces and larches, *the creatures climbed into them and were saved.*" Again, "*Thousands of living frogs also swept* from the fields, no one can say how far off, were observed leaping among the wood" and other wreck thrown up by the heavy rolling tide.

The escape of the sheep was remarkable. But had the trees on which they found a refuge been carried along by a floating mass of drift wood, they must, in all probability have perished: because, not being provided with prehensile organs, or otherwise fitted for an arboreal life, the shaking of the branches, the insecurity of the foothold, and the general terror of the situation, were so many chances against them. Instead, however, of a few sheep, suppose the situation to have been that of the Spider Monkeys, the Ocelots, the Racoons, the Opossums, and the Sloths of America, or the Tapoa Tafas, the Aerial Petaruses, the Pouched Mice, the Sugar Squirrels, and the Tree and Rock Kangaroos of Australia; or the Aye-Ayes and Lemurs of Madagascar. Why! such animals would have been nearly as safe, and as much at home on the unsubmerged part of a tropical raft of wood as the timid sheep in a field of fresh clover. And if we keep these circumstances, as well as others that are obvious enough, in view, we will find that the apparent difficulties to account for the survival of numbers of species are mere mole-hills in comparison with the difficulties that embarrass every attempt to explain the sudden disappearance of the myriads. The survival of a species may mean nothing more than the survival of two individuals, or of a pregnant female; whereas, the extinction of a species implies the extinction of every member of that species. For instance, the extinction of sheep in the United Kingdom would imply the death of twenty-seven millions, whilst the survival of the species might convey nothing more than the escape of a pair.

Yet they all perished—perished over a whole continent; and perished at a date that is written down everywhere by the zone of drift. To allege that they disappeared before a change of conditions is to trifle with common sense. Let us look at only two instances. In America there were two species of Mastodon and two of the Elephant; and the food of these great Proboscidians consists principally of the leaves and twigs of trees: and with this in view, Prof. Owen, speaking of the existing species in Asia and Africa, pertinently asks: "Why should not a third be living at the expense of the still more luxuriant vegetation watered by the Orinocco, the Essequibo, the Amazon, and the La Plata in tropical America?" Well might the distinguished Professor ask the question: Between Cape Horn in the South, and Melville Island in the North, there was every variety of climate, every variety of food; there were boundless forests, boundless plains, they could find a summer paradise on the slopes of the Andes and Rocky Mountains, and on the approach of severer weather descend again to the basins of the great rivers, there to luxuriate in warmth and plenty. In short, in the most favourable conditions in which to live their choice was without limit. They could not have died for want of their natural pabulum; because the fact that the animals which have survived are characteristically arboreal negatives any such idea; and they could not have been extinguished by other animals, and for this simple reason, no other animal survived them that was capable of contending with them.

But the extinction of the Horse over both Americas is calculated to excite our wonder even more than the extinction of these great proboscidians. When America was discovered by Columbus there were no horses on that continent. But the fossil remains of at least one species of the Horse have been found in S. America; and probably three species in N. America. And the fact that three or four species of the Horse were spread over the two great continents manifestly throws the *onus probandi* to account for their complete extermination upon those who deny a general deluge. We have just seen that the two Americas embrace every variety of climate, food, and all the other conditions affecting animal life; and as is well known the seventy-two horses left by the first colonists of La Plata ran wild and multiplied so rapidly that in forty-two years they had spread over the Pampas, and even reached the Straits of Magellan. Doubtless a great authority has told us that the domestic breed succeeded because it possessed more vital energy. Whatever "vital energy" may mean, this imported breed was not acclimatised; it possessed no inherited habits for seeking its food and water; and its dependence on man must have deprived it of those instincts that guide wild animals in choosing the best means of defence against their enemies. To ask us to believe that the European horses, with these obvious disadvantages, succeeded so amazingly, whilst the aboriginal stock of three or four species had died out from want of "vital energy," is to ask us to believe a great deal!

But perhaps the disappearance of six species of the Machairodus is more remarkable still. This "peculiarly destructive feline quadruped" has been found in the Pampas, in the Brazilian bone caverns, in England, on the continent of Europe, and in the Sewalik tertiaries of India. "It was represented by species as large as a lion; and by others of the size of the leopard." (Owen.) Yet six species of this formidable carnivore, armed *cap-à-pie* against all comers disappeared not only from America, but the genus became extinct over the World: Should it be asked how, on the supposition of a general deluge, did the Puma and Jaugar, also large carnivores, escape extinction at the same time, the answer is obvious. The Machairodus was a cave dweller, and this instinct would be against it in a life and death struggle to save itself from drowning; whereas the Puma and the Jaguar are both excellent swimmers and excellent climbers of trees. Either of them, once upon a floating island consisting of a tropical forest, would find itself very much at home; and the myriad of land birds that must have sought such rafts to rest their "weary wing" may have afforded a supply of food.

It is at least remarkable that the living mammalia of Australia present exactly the same distinctive feature from the fossil forms as we have just seen, was the case in the two Americas. In Australia, as in the New World, all the larger mammals are extinct. "The living species," says Krefft, "are of moderate growth, and the largest do not exceed two hundred pounds in weight"; whereas, "many of their predecessors were also Marsupials, and equal in size to the Rhinoceros and Hippopotamus." According to the author just cited, the Phalangers embrace nine genera, and, as the name indicates, they are all arboreal. The genus Antechinus (Pouched Mice) is said to comprise twelve or thirteen species, all arboreal. The Phascogali (at least three species) all arboreal. The Duck-bills and Beaver-rats (five varieties) are essentially aquatic; and the Wombats walk about as well below water as on dry land, and one species frequent the highest trees. The Kangaroos doubtless present the greatest difficulty; but it must not be forgotten that these Marsupials are powerful swimmers, and when pursued take the water from choice. They excel in muscular strength, and can leap farther than other animals. Some of the small varieties can climb well, others find a refuge in the cavities of decayed trees. The Wallaby can leap about the rocks with the agility of a monkey, and we are told that the Tree Kangaroo can clamber about the trunk of the tree, and ascend and descend with the security of a squirrel.

Both the Tasmanian Wolf and Tasmanian Devil are excellent swimmers, and gross feeders, constantly "prowling along the sea-shore in restless search of food among the heterogeneous mass of animal and vegetable substances that the waves constantly fling upon the beach." Manifestly, in an inundation, such animals would have many chances in their favour. But what are the facts? although both survived in Tasmania, they both perished from Continental Australia, that is, they disappeared from a country sixty times the size of the island in which they live to this day. They could not have been exterminated by man, because much of the continent remains unoccupied, and in those regions where the native tribes may be said to be in possession of the land, they have not yet exterminated the numerous species of Kangaroos; moreover, there is no evidence that man was ever in Australia a contemporary of these ferocious members of the Marsupialia. Man, therefore, could not have been the exterminator; and, if we leave man out of account, where is there another possible cause to fall back upon? Certainly not upon the existing fauna, for there is ot a species left in Australia fit to encounter the one or the other.

A failure in the supply of food is equally out of the question, for hosts of graminivorous animals—their natural food—have survived to our day. Besides, the coasts formerly swarmed with Dugongs, Seals, five species of whales, &c., and hence the waves of every succeeding tide must have spread out a never failing banquet to such gross feeders as the Tasmanian Tiger and the Tasmanian Devil.

In considering such a view as I am suggesting, we are apt to dwell upon the difficulties with respect to survivors, and to overlook the numbers that perished. For instance, there are only four known species of living Wombats, whereas, according to Krefft, there are at least twenty species in a fossil state. "A modern Wombat of about 90lbs. weight is considered heavy, but some of the ancient animals, judging from the size of their teeth and bones, must have been more than twice as bulky."

Take again the case of the Kangaroos: the genus Halmaturus is represented in the living fauna by only three species, whilst thirty or more species have been discovered in a fossil state. That is, for every species of this tribe of Kangaroo that survived, ten perished. And in speaking of the number of species, it is well to remember that as regards the living, specific distinctions are too frequently made on very slight grounds, such as difference of colour, &c., whilst specific distinctions, founded on fossil bones, are necessarily based on structural differences. The views that I am advocating suggest another very obvious remark bearing on the number of modern species. Almost every animal is marked by some peculiar feature, which distinguishes it from every one of its fellows. In the wild state, the laws which govern the interbreeding, as well as the general uniformity of the conditions under which they live, all tend to efface this individuality. But suppose only a few of a species, or even a single pregnant female to have escaped from such a deluge as has been suggested, manifestly individual characteristics would not only be transmitted without check, but the novel and alarming surroundings in which each such waif of the sea must have found itself could not have been uninfluential in causing a further departure from the primitive type.

Next to Australia, Papua or New Guinea is the largest island on the globe, and with respect to its mammalia, a writer in Chambers's Ency. states: "In contrast to this variety of birds, is the small number of mammals. The great pachyderms and quadrumana of the Malay Islands are wanting, and excepting two bats and a pig the mammals are all marsupials. There is one true Kangaroo, similar to those of Australia. *The climbing Kangaroos take the place* of the monkeys of the Asiatic area." As regards size, Borneo ranks after Papua, and Murray gives us the following summary :

	Species.
Mammals, Arboreal or Aerial	66
More or less Arboreal	4
Terrestrial	6
Total	76

The terrestrial species embrace four small deer, a porcupine, and a shrew. After calling attention to the fact that the most striking thing is not the greater number of arboreal forms in Borneo, but the absence of terrestrial species, the author continues: "It is as if the number of arboreal species had not been increased beyond what would appear to be the normal proportion under similar conditions; but that the number of the terrestrial species *was diminished*, swept away, or never gained a footing." Of the fourteen species of mammals in the Celebes eleven are arboreal. The three exceptions are the Wild Cow, the Wild Pig, and the Barbirusa, and these were probably all imported by man; and the last is nearly aquatic. Again of the seven mammals in the Timor Group of Islands, four are arboreal, and the fifth—a tiger cat—if not arboreal, is at least an expert climber of trees. The remaining species are a Civet Cat and a Shrew Mouse. The Civet Cat being common over the Archipelago, and frequently made captive by the natives, Wallace thinks it may have been accidentally introduced; and it can hardly be necessary to remark that there is a score of ways in which a mouse may have made an involuntary passage to the islands. We are told by the distinguished author just named that, excluding bats, only ten species of mammals are known from the entire group of islands classed under the general designation of the "Moluccas." These include a monkey, an opossum, and three species of the genus Cuscus, all arboreal. The remainder include a deer, a civet cat, the barbirusa, and a shrew, probably all importations by man. The numerous species of lemurs, monkeys, squirrels, and aye-ayes of Madagascar are all arboreal. The Tenrecs love the neighbourhood of water, whether fresh or salt, and being hibernating animals, they choose a residence in the cavities and among the roots of the bamboos. And hence, on the occurrence of an inundation, there was every probability that out of every 1000 a few would escape.

New Zealand and the British Isles respectively were not very far from the apices of the two great waves of translation, which my views presuppose: with this difference, however, that owing to the great engulfment in the Atlantic the phenomenon was probably much more violent over the latter area than over the former. The effects of this excessive violence have been adverted to; but it is also well worthy of notice that all the tertiary mammals of Great Britain and Ireland were swept away. This is proved by the fact that there is not a living mammal in the British Isles which is not common to the continent of Europe; and not one in Ireland which is not again common to Great Britain; but there are about thirty species in the latter country which have not yet found their way to the former, all going to prove that the existing mammals of Great Britain are importations from the Continent, and those of Ireland from Great Britain.

There are no aboriginal land mammals in New Zealand, and excepting alleged remains of the Dingo, I am not aware that there are any fossil ones; however, there are both living and fossil birds, with wings too rudimentary for flight, and hence, in an inundation of the land, they would run much the same risks as quadrupeds. Rather, the risks would not be quite so great, because some aquatic birds, although they cannot fly, yet they use their wings in swimming and diving; and they even, by striking the water with their pinions, maintain a half-running and a half-flying motion. We have seen how all the large mammals disappeared from both America and Australia; and there are grounds for believing that exactly the same thing has taken place in the case of the brevipennate birds of New Zealand. According to Dr. Haast, the bones of the smaller species of the Dinornis are found in the ancient kitchen middens of the natives, but no remains of such huge creatures as *D. giganteus* or *D. robustus*, and this goes to show that these gigantic forms were extinct before man arrived on the scene. True, there are the traditions of the natives, but it would be rash to accept such authority, unless supported by other evidence. The aborigines of Siberia had similar traditions about birds of monstrous size, which had been exterminated by the intrepidity of their fathers, but unfortunately for the credit of the traditions, the fossil horns of the Rhinoceros had been mistaken for the claws of a bird. But the traditions of the New Zealanders may have had a more respectable origin than ignorance of natural history. *D. elephantopus*, which was probably half the size of *D. giganteus*, is found in the middens: its existence as a living animal has probably been handed down from father to son, and as this kind of lore admits of great amplification, we need not be surprised that in the legendary narratives the bones of the larger birds came to take the place of the smaller ones. The gigantic birds that formerly inhabited New Zealand have now only two puny representatives, the notornis and the apteryx. The notornis being a species of water-hen, its means of escaping an inundation are obvious enough. Owing to its nocturnal habits, all that can be said about the apteryx is, that it conceals itself in holes, and is so powerful that when attacked by dogs "it defends itself so well as frequently to come off victorious." Hence, out of 1000 concealed, say in the cavities of decayed trees, we may take it for granted that a number would contrive to survive. Of course nothing is known of the habits of those extinct forms said to have been the contemporaries of man; but from what is known of their living congeners, it is clear they would be anything but helpless. Ostriches, for instance, take the water both from choice and from necessity: "On two occasions," says Darwin, "I saw some Ostriches swimming across the Santa Cruz River where it was about four hundred yards wide, *and the stream rapid.*" I am not sure whether the fact is generally admitted, but at any rate it has been alleged that the Dingo has been found in a fossil state in New Zealand; whereas the living Dingo was introduced from Australia about two hundred years ago, and should this ultimately prove to be the case, its bearing on my argument is obvious.

These remarks on the change which the deluge effected on the distribution of the terrestrial fauna are already too long: still there are a few circumstances that deserve special attention. Up to the time of the Drift the Lion belonged to the fauna of Europe; but then disappeared. Had its disappearance been permanent, the fact need not have excited our surprise; but what gives significance to the circumstance is that there is no trace of its remains in pre-historic times, whilst with respect to historical times, there is proof that the king of the forest was once more a member of the European fauna. Mr. Boyd Dawkins attributes his first disappearance "to "competition with man, and partly from operation of the same obscure causes which banished the Spotted Hyena and the Hippopotamus." But it is hard to see how this removes the difficulty; because, so far as competition with man was concerned, the Lion would find the contest the more unequal as the population became denser.

Alongside of the simultaneous disappearance of these three great mammals from Europe, let us place that of the little Hamster from England. In Germany, where this fierce little rodent is abundant, it is, we are told, a great pest to farmers, and "the object of their incessant hostility: but it is very prolific, producing two or three broods in the year, and sixteen or eighteen at a birth." Yet, notwithstanding its fecundity and its "more than the pertinacity of the bulldog," this little quadruped, not much greater than a rat but stouter, became extinct in England. Would it not be considered something very remarkable were all the rats in the British Isles to die; whilst those in Germany not only lived but maintained a successful war against their enemies.

Again, the Glutton was a contemporary of the Hamster in England; and it is still an inhabitant of the north of Europe, Asia, and North America as far south as Massachusetts, and for its size it is one of the most formidable of the carnivores. It is a gross feeder, nothing in the shape of meat or carrion coming amiss, either terrestrial or arboreal, whichever for the time being suits its life of rapine, it kills deer, birds, and mice, and even disputes with the wolf and the bear the possession of its prey. Beavers are very frequently selected as its victims, and when its bloodthirsty labours on land or among the branches of trees prove a failure, it takes to the water in search of fish. Now the question may well be put: How did this animal, possessed as it was of so many resources, and so completely equipped for fightiug the battle of life, become extinct at the Drift epoch in the British Isles? And we have an exactly similar case in the Lynx. Both animals became extinct on our side of the Channel, but still live and thrive in Europe, Asia, and America. And their extinction here was synchronous with the extinction of hosts of other animals over the whole world.

We have seen how a great many mammals became extinct over the American Continent, but survived in Europe and Asia; and I will conclude these references by giving two remarkable instances in which the circumstances were just reversed. In the cave and drift deposits of England and the Continent fossil remains of the Musk Sheep and Grisly Bear have been found; but both these animals are now unknown in the Old World, their living representatives being restricted to N. America. It will not do to tell us that they succumbed to climatal conditions, because over the boundless regions of Northern Europe and Asia there was room to gratify every possible choice. Neither can it be said that they were exterminated by stronger and hardier races, for both are notorious for their prowess. It is said of the Musk Sheep that it frequents wild, bleak, and rocky situations, and possesses the agility of the Chamois. "During winter it feeds chiefly on lichens, in summer on grass." (Nat. Ency.) "It is fleet and active, very sure footed on rocky ground, and ascends and descends very steep hills with very great ease. Its powerful horns are great weapons of defence against wolves and bears, which are often not only repelled but killed." (Ch. Ency.)

The Grisly Bear is a more formidable beast still. It is sometimes nine feet long, and is capable of overpowering the bison and dragging away the huge carcase. By nature carnivorous, yet it can subsist on fruits and roots, and being itself a good climber, its victims cannot escape by ascending trees.

Such are the characters of the animals for whose extinction over the whole of Europe and Asia the opponents of the views I am suggesting have to account. Their case is not that of a helpless Dodo—called Dodo for its simpleness—living in a small island, and easily caught and killed. In every point the circumstances were extremely different, and no solution of the difficulty can be satisfactory that does not take into account that both animals not only still live, but they appear to be able to maintain a footing in the world, even in the days of gunpowder and Minié rifles.

SEC. XVI.—THE OSCILLATION DESTRUCTIVE TO MAN AS WELL AS TO THE LOWER ANIMALS.

But if the Drift forms such a bold and well marked line of demarcation in the life of the lower terrestrial animals, what of Man himself?

The question is a fair one, and it is also of very great interest. We have the highest authority for believing that it was because evil abounded that the Creator ceased to preserve the earth; and that Man was consequently the chief sufferer. It is well known that the works of Palæolithic Man are found in caves, under rock shelters, and also promiscuously mixed throughout the body of the drift. In order, however, to learn the lesson which these antediluvian remains are so well calculated to teach, we must take into account some of the conditions in which their owners must have lived and died. Previous to the era under consideration, Europe was an Archipelago, nearer the Equator it is true, but owing to the absence of the Gulf Stream, colder than at the present day. It is also to be remembered that the reaction from severe glacial conditions had only begun shortly before. Hence there is the great probability that the population of the north-west of Europe held the same relation to the more civilised parts of the world that the Esquimaux do to the civilisation of the nineteenth century. If we assume, as I do, that the great waves of translation discussed in my paper produced the Noachian Deluge, its bearing on civilisation is obvious. To whatever degree human knowledge may have advanced, even in the most civilised part of the world, it is clear that the arts and sciences not possessed by four families must have perished. True, these families, owing to their piety and long lives, probably knew the most of what was worth knowing. Still, before their descendants multiplied to the extent which the colonisation of Western Europe presupposes, the arts must have assumed types quite distinct from those of the people who perished miserably amidst the wrath of the diluvial waters. Now, just as might have been expected, the remains found in and under the drift are marked by characteristic features which readily distinguish them from those above it. So clear is the line of separation that archæologists have been able to divide the Stone-age into two periods: the Palæolithic or First Stone Age; and secondly, the Neolithic or Second Stone Age. In his Prehistoric Times, Sir John Lubbock adverts to the distinction between the two periods as follows:

> "Again the flint implements of the drift are never polished or ground, but are always left rough. We may safely estimate that three thousand at least have been already found in the drift gravels of England and France, and of this large number there is not one which shows a trace of polishing or grinding, while we know that the reverse

was almost always the case with the Celts of the later stone period. It is true that the latter is not an invariable rule; thus in Denmark there are two forms of so-called axes which are left rough: namely, the small triangular axes of the Kjokkenmoddings, which are invariably so, and the large square-sided axes with which this is often the case. But these two forms of implements resemble in no other way those that are found in the drift, and could not for a moment be mistaken for them. It is not going too far to say that there is not a single well authenticated instance of a celt having been found in the Drift, or of an implement of the drift type being discovered either in a tumulus, or associated with the remains of the later Stone Age."

Here we have a line of separation that cuts with all the sharpness of a steel blade, and what greatly adds to its significance is the fact that there was no change in the material from which the implements were manufactured. Had the transition been from stone to bronze it might have been alleged as the reason why they never by any chance got mixed, that the superiority of metal so approved itself to everyone, that it all at once superseded the use of stone. But no such explanation is admissible here: or rather, all the facts point just in the opposite direction. For instance, the use of stone implements continued long after the introduction of bronze, and implements of this metal even appear to have been made after flint models. In the same way the age of bronze is gradually dovetailed into iron. In fact the sharpness of the line that divides the Stone Age into two, forms a complete contrast to the gradual shading off into each other, which is a universal feature of the subsequent periods; and it could have been no local cause that produced such an abrupt and universal break in the continuity. And, manifestly the date of the break cannot be dissociated from the Drift. This is clear for three reasons: first, up to the close of the Tertiary period all the lower lands of the North of Europe were submerged, and, as might have been expected from the fact, there are no implements of the Palæolithic type over those regions; secondly, in Denmark and Sweden flint implements are found in deposits immediately overlying the drift, but are all of the Neolithic type; and thirdly, in countries where the Palæolithic forms exist, they are found under and in the drift, but in no deposit superior to it. On my hypothesis the people perished with the extinct mammalia, and here again the facts come to the aid of theory. "Nor do the types of Neolithic Age," says the distinguished author just cited, ever occur in company with Quaternary fauna, or under circumstances which would justify us in regarding them as coeval." And if Palæolithic Man disappeared simultaneously with the Quaternary mammalia, then in that case those who object to the views for which I am contending, have much to account for. But we must go back to Palæolithic Man. From the very successful labours of M. M. Christy and Lartet we learn that in the First Stone Age considerable progress had been made in studying the beautiful; that Palæolithic Man not only knew the happy use that could be made of curves, but that he actually spent his leisure hours in making fair sketches of animals; and we are told that even shading was attempted. Now in perfect contrast to this, Neolithic Man appears not only to have been ignorant of the art of sketching, but of the use of curves altogether. Ancient tumuli contain numerous varieties of pottery, and many of the cups, vases, and coins are elaborately ornamented with combinations of straight lines, but no attempt is made to embellish them with curved lines or figures, so superior in every respect when we are aiming at the beautiful, and the idea of using sketches of either animals or plants appears to have never entered the heads of those early predecessors of Palissy and Wedgewood. And it is not to be forgotten that this state of Neolithic art not only continued to exist down to the close of the Second Stone Age, but also to the time when the Romans are supposed to have introduced their arts and sciences into the West of Europe: that is, this crude system of ornamenting works of art was in use throughout the whole of the Neolithic and Bronze Periods.

How then, I am entitled to ask, was the use of curved lines and the art of sketching and shading of the First Stone Age lost to Europe? Surely the more numerous, and as we are often enough told, the more civilised people of the Lake Dwellings ought not only to have preserved the art, but to have improved on it as well: but instead of their doing this, the very idea of the thing appears to have been unknown to them. It will not do to say that the inhabitants of the Dordogne were probably exterminated by a less advanced race; for even if we grant this, and grant further that the invading horde was ignorant of the contemporary art—the presumption still remains that such a new and happy idea as the picture of an animal must have arrested attention and produced imitators amongst the most barbarous people; and the presumption becomes stronger when we remember that the pictures were drawn on enduring material. Thus without a break in the occupation of Europe by Man, it is, as appears to me, impossible to explain how the use of curves and the art of sketching was so completely effaced. At any rate the *onus* rests with those who reject these views to account for the remarkable circumstance that men suddenly ceased to practice an art which must have afforded them pleasure, and replaced it by the much inferior system of ornamentation by straight lines.

To recapitulate, we have: First, the Palæolithic antiquities are always found either mingled promiscuously with the undisturbed drift, or under the strong natural protection afforded by caves or rock-shelters, whilst, and in perfect contrast, all the antiquities belonging to the Neolithic age are found either resting quietly on the surface, or in ancient dwellings, tumuli, and kitchen middens; just the places where we would have naturally looked for them; all going to prove that the sharp line that cuts the Stone Age into two is due to some physical cause that must have been universal in its range and complete in its effects. Secondly, the obvious synchronism that exists between the extinction of the Quaternary mammalia and the disappearance of Palæolithic Man can leave no doubt that both results were due to a common cause. And Thirdly, the loss of the art of sketching pictures. &c., simultaneousiy with the abrupt disuse of the Palæolithic type of implements is simply inexplicable on any other supposition than that of an interregnum, during which Europe remained uninhabited by Man.

Every one of the above considerations, taken even by itself, lends strong support to my contention: and when the concurrent testimony of the whole is taken into account the proof becomes as strong as anything short of positive demonstration well can be. Sir John Lubbock holds, it is true, that the gravels in which the Palæolithic implements are embedded were deposited by ordinary river action, and in support of his view adduces the fact that the stones peculiar to the watershed of the Somme do not extend to the valley of the Oise. But any one who has done me the honour to wade through what I have said on the cause and direction of the great waves of translation will see that Sir John's argument has no force, at least so far as my case is concerned: because, although the two rivers in question approach each other at one point, yet their courses are nearly at right angles to each other. Speaking generally, the Somme has a north-west direction, and consequently a wave of translation from the north would cross the watershed, simply washing the shingle from the north side into the river valley, where it would leave it; whilst on the other hand, the course of the Oise is south-west, and hence there was every probability against the shingle of the one watershed being washed into the other.

The view that the valley gravels are simply the result of river action is not, however, held by all geologists. Mr. Taylor, who has given so much attention to the subject, writes of the Amiens gravels: "That the surface of the chalk in the valley of the Somme had assumed its present form prior to the deposition of any of the gravel or loess now to be seen there, and in this respect corresponds with all other valleys in which Quaternary deposits of this character are met with; that the whole of the Amiens valley gravel is of one formation and of similar mineral character. And that the gravel in the valley of the Somme at Amiens is partially derived from debris brought down by the Somme and by the two rivers, the Celli and the Avre, and partly consists of material from the adjoining higher grounds, washed in by land floods, the immense quantity of chalk being derived from the latter source." * * * * "That many of the Quaternary deposits in all countries, clearly posterior to the formation of the valleys in which they lie, are of such great dimension and elevation that they must have been formed under physical conditions very different from our own." * * * * "That there is evidence of very little weathering or atmospheric action since the date of the drift, and that the upper and lower valley gravels in the Somme are continuous and of one period."

It is also fortunate that the gravels under consideration have been examined by Dr Andrews of Chicago, who has done so much to unravel the drift phenomena of N. America, and his testimony cannot be misunderstood. "Sir John Lubbock believes," says Dr Andrews, "that the gravel was an extremely slow product of deposition, occupying untold ages in its formation. He contends that it was gradually formed by the river while it was slowly excavating the valley from the chalk, and that the stream at that time had but little greater annual quantity of water than it has now; but that its flow was less even, more of being concentrated into the Spring floods. In the gravel pits ne

Amiens I observed some facts which have an important bearing on the question of time. There is evidence there that at the time the deposit was formed blocks of ice, or of mixed ice and frozen gravel three or four feet in diameter, *were laid down in the strata, and that these blocks were completely covered and had other strata laid above them before they had time to melt.*" * * * * "This mass then in some way disappeared, allowing the clay stratum to settle and break, and the upper gravel to pass in and occupy the vacancy. What was the temporary mass? All the circumstances indicate that it was a block of ice, or at least of frozen gravel and ice combined, with enough of the latter to leave a vacancy when it melted. An examination of the grey gravel adjacent confirms the idea of powerful action. It consists of chalk flints, mixed with broken chalk of every size, *from a fine powder* up to fragments as large as a man's head. Many of the fragments though *soft enough to write with on a black board, have preserved with absolute perfection the sharp angles and edges* which they had at the time they were broken from the cretaceous strata. It does not seem possible that they could have been rolled one hundred feet in the bed of a stream without losing this sharpness."

Such facts as these appear conclusive against the view that the gravels of the Somme were deposited during the gradual excavation of the river valley out of the chalk. However, as extravagant speculations, as to the antiquity of man have been founded on the assumption that what is known as the high level gravels were deposited by the rivers before the valleys were formed, it may be well to advert to the position in which the early type of implements are found in the English gravels.

"Mr Prestwick observes," says Mr Flower, in a paper read before the Geological Society, "that it would be impossible for the present rivers even during their greatest floods to attain a height at all approaching to the high level gravels, and suggests that from the melting of the snow independently of any larger rainfall, the floods must formerly have been far greater, and have given to the rivers a torrential character, and that thus the ancient channels have been deepened. But, as applied to Thetford, the phenomena in question can hardly be accounted for on this supposition; snows do not give forth torrents of water sufficient in volume and force for the transport of such deposits as are here exhibited, unless in regions, at least to some extent mountainous, and it is impossible to find any mountains, or indeed any high land, in the narrow water-shed of the Waveney and the Ouse, and to affirm that some such district once existed would be a gratuitous assumption remitting us to the region of pure conjecture." * * * * * "Here has not only all the boulder clay on the one side of the valley, and to the extent of three-quarters of a mile in breadth on the other side, been carried away, together with large masses of the chalk on which it once reposed, but vast masses of the flint gravel which doubtless once covered the adjacent hills (and of which extensive traces still remain) have also been removed."

As on the Somme, the implements were found from twelve to fifteen feet below the surface, and "*within a foot or less of the chalk.*" And Mr. Flower concludes that the contents of the valley must be explained in some other way than ranking them as river deposits, "especially as they bear unmistakable marks of violent action." In another memoir Mr Flower calls attention to the position of the implements: "Where they occur in profusion, as at St. Auchenland, Thetford, and some other places, they are seldom found in the over-lying laminated sands and gravels, but are seen to rest immediately on the chalk or other subjacent rock. Had they been brought down by freshets or river floods we should expect to find them in layers indicating alternate periods of repose and disturbance; whereas their actual position leads rather to the belief that they were all transported at one and the same time, together with the drift gravel and sand lying loose upon the surface into the then existing hollows and valleys."

In Vol. 28 of the Journal of the Geological Society there is an important paper on the Palæolithic implements in the gravels of the Thames at Acton, by Colonel Lane Fox, and as the facts appear to settle the question, I take the liberty of making a few extracts:

"Upon inquiry I found that the cuttings for the sewer were carried much deeper than the foundations of the houses; and the implements must therefore have been brought up from the very bottom of the gravel bed, thus *confirming in a remarkable manner* the experience derived not only from Acton, but from other similar deposits of drift gravels, in which *the implements are all found to lie almost invariably at the bottom of the gravel.*" * * * * "Here [the lowest stratum of gravel] the largest flint stones lie, and with them the implements, mostly of the dimensions of the larger stones, so that it was common for the most experienced workmen to say that they should find no implements till they got down into the coarse gravel: *the smaller flakes were, however, not so invariably at the bottom.*" * * * * "And at the bottom of all (in the mid terrace) in proximity to the London clay, a thick layer of animal remains, mixed together and lying invariably at a level of 12 to 13 feet from the surface." * * * * "Throughout this district, over probably more than half a square mile in extent, wherever the London clay is reached, animal remains in great abundance are found above it, and always, I believe, in close proximity to the clay." * * * * It may, I think, be stated as a fact, that no flint of the drift type in this region has hitherto been found below the 50 feet line except in the bed of the Thames itself. From the Thames, I have obtained one implement of the drift type; and Mr Sparrow Simpson is in possession of a remarkably fine specimen, also from the bed of the Thames."

"The presence of these implements in the high terrace, their absence so far as our researches go, in the mid terrace, and their re-appearance again in the present bed of the Thames—the abundance of animal remains in the mid terrace, and their great rarity, if not absence in the high terrace—*and the invariable occurrence of both implements and animal remains in the lowest stratum of the gravel* immediately adjoining the London Clay are facts which demand an explanation of some kind. * * * * "The presence of large tracks of brick earth over-lying the gravel, argues, as I venture to think, the existence of large volumes of water at the time they were deposited. Then to what cause are we to attribute the stripe of London Clay laid bare on the sides of the valley and on the tributary streams, at the average level of 50 feet. Obviously to denudation of some kind."

Looked at as a question of evidence, the facts and circumstances given in these extracts cannot be over-rated. The bones in the mid terrace being neither water-worn nor rolled proves that they were floated to their present position; and this again that the water in the estuary at Acton then stood some 60 or 70 feet above its present level. For obvious reasons the first adventurers who seek a home in an uninhabited country select a bay on the sheltered shore of an estuary for their settlement. At Acton all these advantages would be secured with the additional one that the exposed chalk along the high water line afforded them the raw material in abundance for the manufacture of their tools. Implements made of iron being comparatively prominent objects are less likely to be lost than those made of stone: and when actually lost the metal soon oxidises and disappears. On the other hand stone implements, from their likeness to surrounding objects, are not only liable to be lost, but the enduring character of the material preserves them to all time. Hence we may conclude that at a settlement where flint was abundant, stray implements would year after year accumulate along the shore of the estuary. And both before and after the advent of Man the river doubtless brought down the carcases of animals accidentally drowned or killed; and such remains would come to final rest either in the deep pools or in nooks and bays sheltered from the action of the currents and breakers. In this way the animal remains would be collected at a lower level than the stray implements and flint chippings: and on the supposition that a great wave of translation passed over the country from the north, the gravels swept off the higher grounds on the north side would cover up the implements and mammalian remains very much in the position where they lay. Another noticeable circumstance is that the smaller flint flakes are not so invariably at the bottom of the gravel as the implements, and this again is just what might have antecedently been expected, as obviously the process of chipping the flints would not from choice be carried on in low, marshy situations, but on dry knolls and banks; and, moveover, in the disturbance caused by the first tumultuous approach of the torrent, the lighter flakes would be more knocked about than heavier flints.

With the simplicity of this explanation contrast the difficulties that beset the view that the valley of the Thames has been excavated since the high level gravels were deposited. Colonel Lane Fox gives a section across the valley from Acton to Richmond Park, which shows that the distance between the eighty feet level on the two sides of the river is about four miles. The first observation which this suggests is that an elevation of the bed of the river to the eighty feet level at Acton would, for a distance of about thirty miles, take away the whole of the present fall. And how can we bring our minds to believe that a river without any fall for many miles above the point in question deposited gravels to the depth of nine or ten feet. Rivers without a fall do not at the present day transport gravel at all; and what adds to the improbability of the whole thing is that the hypothesis requires us to suppose that the gravels were deposited at a time when the river was deepening its channels; but rivers without sufficient fall do not deepen their channels at all, but rather tend to fill up the valleys through which they flow.

The notion cannot be entertained that the rivers when flowing at the eighty feet level filled the whole valley: in other words, that the river was four miles broad. But if the river did not fill the valley, and if the valley be a valley of erosion at all, it is clear that the channel must have often changed. We know the works of Man were on the spot before the deposition of the high-level gravels, and we also know that numerous remains of the extinct mammalia underlie the low-level gravels. Further, the hypothesis requires us to believe that both the high and low-level gravels were deposited on what was for the time being the bed of the river, and that these depositions represent the time during which the river excavated a valley four miles broad and some sixty or eighty feet deep, for there are typical flints in the present channel of the Thames—the river being, during at least the early part of the period, without fall.

These circumstances suggest one or two obvious questions: First, how did it come to pass that during the long period represented by the high-level gravels only the works of Man were accumulated in the bed of the river? Again, how did it happen that during the long period represented by the low-level gravels the remains of Man were not deposited, but in their room a great abundance of the extinct mammalia? Thirdly, by what series of fortuitous events did it happen that both classes of remains were deposited only at the base of the gravel, for it is not to be forgotten that the theory takes for granted that the river continued to flow over the spot till it covered them up with some ten feet of gravel. And fourthly, by what other series of accidents were the flints themselves divided into two zones: the heavier implements taking up their position at the base and the flint chippings farther up in the gravel. Had the position of the implements and the mammalian bones been transposed we would doubtless have been told that the reason why the remains of the quadrupeds were under the higher gravels was that they preceded Man in the South of England, and that their disappearance from the lower level where the implements took their place was owing to the exterminating zeal of the hunter. Again, had the implements been all at the same level above the sea, it might have been said that a Palæolithic London plague cleared the land of its human inhabitants; but unfortunately for the supposition they are not all at the same level, and hence this suggestion will not help us out of the difficulty. Neither can it be alleged that a great river flood suddenly covered up the remains with shingle; because, according to the hypothesis the lower part of the valley where the mammalian bones are deposited was not excavated at the time the implements under the high-level gravels were covered up. In addition to all this the true Brick earth invariably over-lies the gravels, pointing to a much broader agency than river action.

Moreover, it cannot be too strongly insisted on that exactly the same features characterise the drift in the Somme valley, the Norfolk Ouse, and the Thames. In all the three valleys there is the same order in the superposition; the drift gravels over-lying the implements, and the gravels in their turn being over-laid by the Brick earth or Loess. Now a glance at a map of Europe will show that all the three rivers in question are east and west in their direction, and hence a wave of translation from the north would cross their respective courses. In such circumstances the first rush would disturb and drive hither and thither any deposits in the river valley and along the banks, but so soon as the valley filled with water the onward current would pass over head merely sweeping in the shingle and heavier debris from the higher grounds. And keeping these obvious conditions in view, let me now solicit attention to the circumstances and surroundings generally under which implements have been discovered by Dr Blackmore, at Milford, near Salisbury.

In a paper on the subject read by Mr. Evans to the Geological Society of London, we are told: "The drift at Milford completely invests the hill. It is thickest at the top where it attains a thickness of from 10 to 12 feet, thins out gradually on the sides, and ceases altogether more than half way down. * * * * It is without stratification and rests upon the chalk. It must be evident to any one that when the gravel was deposited on Milford Hill the ancient river must during continued variations of its course have extended from Laverstock Hill on the east to Harnham Hill on the west, a distance of about three miles. * * * * *The implements were found scattered unevenly throughout the deposit; the majority, however*, occur low do
and in many cases embedded in the chalk rubble above mentio

They are of the long pointed type, thus confirming the opinion of Mr. Evans that this particular form is mainly characteristic of gravels. The majority are water-worn and shew evident traces of having travelled in very rough company, and bear marks of many a hard knock and jostle by the way; others, however, have the angles of the chipping as sharp and well preserved as if they were made but yesterday. Some were stained of a deep yellow colour, others only partially, and some not at all. It is rather remarkable that the staining does not appear *to be due to their present position in the gravel*, as some of the darkest coloured ones have been dug out of the pale chalk rubble, side by side with fragments of flint retaining its original hue, and on the other hand perfectly unstained examples have been obtained from the dark ochreous gravel."

Here we have three things deserving attention. As regards the "finds" in the three river valleys discussed above, we have seen that they were at the base of the gravel, and not unfrequently rested immediately on the chalk; whereas at Milford Hill, whilst it is true that they are more numerous towards the base, yet a great number of the implements are mixed throughout the body of the gravel. The second instructive particular, and which may be looked upon as a complement to the first, is that the majority of the implements are water-worn, and "show evident traces of having travelled some distance in rough company, and bear marks of many a hard knock and jostle by the way." North of Salisbury the course of the Avon is almost due south; and hence the first advance of such a wave of translation as I have advocated would be in the form of a torrential current rushing down the ordinary water-shed. The momentum and consequent propelling power of water in motion is made up of its depth and velocity; but when considering the ploughing-up effect of such a current, we must not over-look an essential distinction between it and an ordinary river current. During the course of ages a river works out for itself many adjustments; and its waters always have a supporting body of water in front, but in the case of a wave of translation another factor enters the calculation. The water, as it were, moves forward in a body without any similar wave in front to retard or moderate its forward rush, and consequently the law which governs the velocity with which water spouts from an orifice in the side of a vessel comes into action. That is, the force downwards increases as the square root of the depth. Suppose, for instance, the advancing column to have had a depth of 25 feet, then in that case the force at the base must have been five times that at the surface. This being so, we may take it for granted that on such a torrent rushing down a river valley, the gravels and ancient alluvium would be ploughed up and forced along to points where from the widening of the channel or obstructions in front the momentum lost its force. Now keeping this condition in view we find Milford Hill just in the position where we ought to have expected an accumulation of drift-gravels. It is on the left bank of the Avon, and in the vertex of the triangle formed by the meeting of this river with its affluent, the Bourne. On the north side it is sheltered by Harnham Hill, to which it forms a sort of a spur; but as the position of the gravel on the hill may suggest a difficulty, it may be well to interpolate an explanatory remark or two. If we slightly disturb water in course of running out of a lavatory basin, a violent vortical motion is at once established. In such an experiment, gravity produces a current from the circumference to the centre of the vessel; but after the disturbance, the current does not, at all points, reach the centre with the same momentum. This again causes a rotatory motion, and the centrifugal force thereby generated prevents the liquid from filling up the vortex-cavity which is, of course, over the plug hole. Comparing small things with great, a whirlwind is just such a vortex upside down. Two currents of air strike against each other, they cannot pass, there is no escape laterally, and as both press on with a momentum extending perhaps for hundreds of miles, the air is forced to ascend; and this ascensional current once established, the air rushes in on all sides to the vortical centre, producing the destructive cyclone.

Now the behaviour, towards each other, of two currents of water must, in kind, be exactly the same as two currents of air. But water is 810 times the specific gravity of air, and hence any ascensional current formed by the collision of two moving columns, must have swept stones, gravel, and everything movable into its vortex. If the two columns met in a valley or basin, walled in by hills, the scene of the collision must be marked by a mound; but suppose this meeting to have taken place in an open situation, and at an angle, then the vortical current must have run along the line of impact, forming ridges and knolls in its course. Of the latter, the Kaims of Scotland, and the Eskars of Ireland afford good examples; and the drift on the top of Milford Hill is doubtless a specimen of the former; that is, of the effect of the more localised kind of vortical motion.

The third characteristic deserving attention is, that the staining of the implements is not due to their present position in the gravel. This, I need hardly say, is in perfect agreement with the view for which I am contending; but if we attempt to explain it on the supposition that the gravels were deposited by ordinary river action, we are met with great improbabilities. To assume that the top of Milford Hill was the bed of the river would require us to assume farther that, when the gravels were deposited, the Avon was enormously broader than at the present day; but a broader channel and a greater elevation mean a greatly reduced gravel-carrying power. Let us, however, grant that the expanded river still had sufficient momentum to transport gravel, the puzzling question remains as to the source from which the implements came. It cannot be argued that they were placed where they now are by man; and if not, they must, in some way or other, have been drifted thither by water in motion. The fact that about one-fourth of the whole look fresh, with their edges sharp as if newly chipped off, puts ordinary river action out of the question; because a very short exposure to the triturating process in the bed of a river must have worn off the sharp edges. On the other hand, implements borne along by a torrential current that swept the valley of its gravels, and old alluvium would become, as it were, a part of the mass; and this would give many of them a chance of passing through the ordeal of their sudden removal without having their sharpness rubbed off.

Again, the staining of the implements represents a protracted period, and as the stained specimens are promiscuously mixed through the gravel, we have in the circumstance strong presumptive proof that the whole mass was deposited at the same time. The staining could not have taken place under water, as in that case, the implements must have suffered from tear and wear like other river shingle, and the difficulty will not be got rid of by alleging that the unstained specimens may have travelled but a short distance in the bed of the river; as then, the light coloured ones ought to have the least evidence of water wear, and, speaking generally, ought to be proportionably more numerous at the base of the gravel formation; but the actual facts lend no support to any such idea. Dismissing therefore the notion that the staining took place in the bed of the river, we must fall back on the only other alternative, namely, that it happened on dry land: or more probably in the marshy alluvium and decayed vegetable matter that form a fringe to rivers in their natural state. That it was in such positions the staining took place, I have no doubt whatever; but then on the fashionable theory, this does not bring us one iota nearer to a solution of the problem. Without dispute the staining process represents time; and as it was done on land, or, at any rate, in marshy spots above the level of the river—of a river, be it remembered, which, according to the premises, was deepening its channel—it is manifest that all the conditions were in favour of, season after season, placing the implements higher and drier beyond the reach of fluviatile action. There is, of course, the possible supposition that the river might have undermined its banks; but this would imply that the flints fell to a lower level, and would only increase the difficulty as to how they got to the top of Milford Hill.

But perhaps the strongest argument in support of the view that the valleys were excavated before Palæolithic Man first dropped his flint tools at the spot, is the fact that the tools are there at all. On the hypothesis that the valley has been formed since man first frequented the banks of the Avon, there is the question as to the source of the raw material out of which the implements were manufactured. Before the river cut an escarpment in the chalk the flints could only have been got by the slow, and, to the hunter, irksome process of quarrying; but, as is well known, the implements were made from flints that had previously been water-worn, and hence, quarrying is out of the question.

We may therefore assume that the pioneers in Salisbury Plain, found, in a river valley running through a chalk district, the flints suitable for their purposes, and the case of the Avon is in strict harmony with the fact that all the other great "finds" of implements have been discovered in similar situations. From the nature of the material a large proportion of the flints would be rendered useless during the process of chipping, and for this reason the manufacture was doubtless carried on at places where they were plentiful. In course of time some were dropped and lost, others were left, and subsequently forgotten: or the hunter, or warrior may have secreted his store just before starting on his last expedition. In any one year the number of such waifs may have been trifling; but when the great wave of translation swept down the valley, it may have unearthed the accumulations of ages; and the various degrees of staining which the implements have undergone prove that such was the case.

If we weigh well the above circumstances we cannot, as appears to me, avoid concluding that, on his first arrival, Man found the valley of the Avon already there, and that it was for the obvious advantage which it offered that he chose it for his home, or at any rate, for a haunt to which he could resort for his supplies of flint tools. But if the valley was formed before the remains of Man's art were dropped in the matrix where they were stained, then we must take a further step, and conclude that their present position in the gravel on Milford Hill is due to a broader agency than river action.

SEC. XVII.—THE EFFECT OF THE OSCILLATION ON THE DISTRIBUTION OF PLANTS.

But if the great waves of translation destroyed, as I have contended, so many mammals and even Man himself, it may well be asked, what influence had they upon the Floras of the world? Manifestly the floating rafts of trees, which the hypothesis takes for granted, must have carried innumerable plants and seeds to great distances; and the birds which found, in such rafts, so many places of refuge, must have been another means by which seeds became widely distributed. Again, masses of ice, suddenly torn from their birth-place in high latitudes would carry Arctic and Antarctic species into warmer climates. To show that such a distribution of plants actually took place, I shall cull a few facts from Darwin's Origin of Species :—"In South America, Dr Hooker has shown that, besides many closely allied species, between forty and fifty of flowering plants of Tierra del Fuego forming no inconsiderable part of its scanty flora are common to N. America and Europe, enormously remote as these areas in opposite hemispheres are from each other. On the lofty mountains of equatorial America a host of peculiar species belonging to European genera occur." * * * * Dr Hooker has also shown that several of the plants living on the upper parts of the lofty island of Fernando Po, and on the neighbouring Cameroon Mountains, in the Gulf of Guinea, are closely related to those on the mountains of Abyssinia; and likewise to those of temperate Europe." * * * * * "The same temperate plants have been discovered on the mountains of the Cape de Verde islands. * * * * on the Himalaya, and on the isolated mountain ranges of the Peninsula of India, on the heights of Ceylon, and on the volcanic cones of Java. Many plants occur, either identically the same, or representing each other, and at the same time representing plants of Europe not found in the intervening hot low lands. A list of the genera of plants, collected on the loftier peaks of Java, raises a picture of a collection made on a hillock of Europe! Still more striking is the fact that peculiar Australian forms are represented by certain plants growing on the summits of the mountains of Borneo. Some of these Australian forms, as I hear from Dr Hooker, extend along the heights of the peninsula of Malacca, and are thinly scattered on the one hand over India, and on the other hand as far north as Japan." * * * * "The Alpine plants, for example of Scotland, as remarked by Mr. H. C. Watson, and those of the Pyrenees, as remarked by Ramond, are more especially allied to the plants of Northern Scandinavia; those of the United States to Labrador; those of Siberia to the Arctic regions of that country." * * * * "Various special difficulties remain to be solved; for instance, the occurrence, as shewn by Dr Hooker, of the same plants at points so enormously remote as Kerguelen Land, New Zealand, and Fuegia; but icebergs, as suggested by Lyell, may have been concerned in their dispersal."

The above facts are highly interesting; and to explain them the distinguished author falls back on the theory of alternate glacial periods in the two hemispheres; but how this explains the passage of boreal species across the Equator is more than I can see, and if we grant the climatal conditions favourable to the passage of the northern species and *vice-versa*, we appear only to raise the more difficult question as to what became of the tropical flora meanwhile? Now on the view that I have advocated, the wide distribution of plants at the close of the Tertiary period proper, is just what, on theoretic grounds, we ought to have expected. Because, on the one hand, the N. Atlantic diluvial wave must have carried the boreal species as well as the European and N. American forms to the south; and on the other hand, the S. Pacific wave must have transported those of Victoria Land in the direction of Kerguelen Land, New Zealand, and Cape Horn, and the same wave in its progress northwards must have stranded the plants of Australia, on the mountains of Borneo, Malacca, and India. For it is not to be forgotten that Victoria Land, on my hypothesis, was probably 10° nearer the Equator, with the sea standing 1800 feet higher at New Zealand. Also, it is not to be forgotten that although the great rush of the diluvial waves was necessarily parallel to the meridians, yet there must have been lateral currents as well, towards the lines of minimum effect. Again, and as already remarked, each wave after passing the Equator would flow into a converging space, and this would increase its depth, communicating a tendency to spread out fan-like into the other hemisphere; and, if we keep this in mind, we will not be surprised to find an affinity "between the flora of *the south-western corner of Australia* and the Cape of Good Hope."

SEC. XVIII.—THE ACCOUNT OF THE DELUGE, AS GIVEN IN THE ENGLISH BIBLE.

Most of us, I am afraid, have only vague and very indistinct notions of what the Bible really says about the Deluge. It may even be doubted whether we give it the right name: for the Hebrew word *mabbul* is used only with respect to the Noachian Deluge, and this fact leaves some uncertainty as to its meaning. The Seventy translate it by "cataclysm," a word that has several shades of meaning, The verb, according to Liddell and Scott, means "to dash over; deluge; flood; to fill full of water; to overwhelm; to make life overflow with plenty; to deluge with sufferings; to deafen with words; to wash down or away; to wash out." In all this it will be observed the idea of rain never comes in; and it is also a noticeable fact that deluge is from a Latin word 'to wash.' Job makes the following striking reference to the Deluge: "Or who shut up the sea with doors, when it burst forth as if it had issued out of the womb? When I made the cloud the garment thereof, and thick darkness a swaddling-band for it; and break up for it my decreed place, and set bars and doors, and said, Hitherto shalt thou come, but no further, and here shall thy proud waves be stayed?" Still not the most remote reference to rain. The first mention of the impending judgment is in Gen. vi. 7: "And the Lord said: I will destroy man, whom I have created, from the face of the earth; both man, and beast, and the creeping thing, and the fowls of the air; for it repenteth me that I have made them." The word *Machach*, here translated "destroy," ought to have been rendered "blot out, wash out, or wipe out." The same word is in 2 Kings xxi. 13, "I will wipe Jerusalem as a man *wipeth* a dish, wiping it and turning it upside down." Moses ys, "If not, *blot* me, I pray thee, out of thy book." In verse 13 of

Genesis vi., we have: "Behold, I will destroy them with the earth." Here the original *shachath* for destroy is correctly rendered, and its meaning is destruction of an absolute or a violent character. And it will be observed that man and the earth were to be involved in a common destruction. It is also noticeable that it is the same term *shachath* which is used in describing the destruction of Sodom and Gomorrah. Chapter vii. 4: "For yet seven days, and I will cause it to rain upon the earth forty days and forty nights; and every living substance that I have made will I destroy (wipe away or blot out) from off the face of the earth." Here we have the verb to rain used for the first time; but it is not natural rain. The Hebrew *matar* is in the fifth conjugation; and in this form when it does not refer to the withholding of rain, it means to rain something special. For instance: "The Lord rained upon Sodom and Gomorrah brimstone and fire from the Lord out of heaven." * * * "Behold, to-morrow about this time, I will cause it to rain a very grievous hail." * * * * "I will rain bread from heaven for you." * * * * When he is about to fill his belly, God shall "rain" the fury of his wrath upon him while he is eating (Job xx. 23).

The references to the deluge adverted to above were only so many premonitions of what was coming; but we have now reached the history of the dreadful event.

"The same day were all the fountains of the great deep broken up, and the windows of heaven were opened. And the rain was upon the earth forty days and forty nights."

The question that meets us here on the threshold is: What is the meaning of "fountain?" Moses, so far as I am aware, uses the word only three times; twice in connection with the Deluge, and again in Lev. xi. 36; "Nevertheless, a fountain or pit, wherein there is plenty of water, shall be clean; but that which touched their carcase shall be unclean." It will be observed that the question as to whether the water is to be considered clean or unclean is made to depend upon the quantity in the "fountain or pit." Now, "fountain" in this instance, can hardly mean what we call a running fountain or spring, because living waters are always clean. It therefore seems that here, at least, the Hebrew word *mayan* might be rendered basin or reservoir. The next occasion on which the word is used is with respect to the borders of the Children of Judah (Joshua xv. 9): "And the border was drawn from the top of the hill unto the *fountain* of the water of Nephtoah." As it stands, this is a singular expression. Nephtoah means "opening," so it reads, "fountain of the waters of the opening." In Judea the waters of a spring are not allowed to run to waste, and basins or reservoirs are made to collect them, and when we substitute basin for fountain the passage before us becomes, as appears to me, more intelligible. And in this case the basin would be the place where the waters of the opening were collected. The translators themselves seem to have been in some doubt about this word, for in Chap. xviii. 15, the same spot is called "the well of waters of Nephtoah." When Achsah asked her father Caleb for a field she complained (Joshua xv. 19) "For thou hast given me a South land; give me also *springs* of water. And he gave her the upper *springs* and the *nether springs*." It cannot be doubted that natural springs are spoken of here, but the original is *gullah*. Solomon tells us (Prov. xxv. 26): "A righteous man falling down (moved) before the wicked is as a *troubled fountain*, and a corrupt spring." Here the words "troubled" and "corrupt" are suggestive of a fundamental distinction between fountain and spring. "Troubled" means fouled, as in Ez. xxxiv. 18. "Seemeth it a small thing unto you * * * to have drunk of the deep waters, but you must *foul* the residue with your feet;" whereas corrupt spring means something radically evil. "All flesh had *corrupted* his way." (Gen. vi. 12.) "But cursed be the deceiver which hath in his flock a male, and voweth, and sacrificeth unto the Lord a *corrupt* thing." (Mal. i. 14.) The fountain, it will be observed, could be trodden and fouled by the feet; but the corruption of the spring was of a deeper and more incurable character. In the Song of Songs we have the following two passages (Chap. iv. 12): "A garden inclosed is my sister, my spouse; a spring shut up, a fountain sealed." (Verse 15) "A fountain of gardens, a well of living waters, and streams from Lebanon." Thus "fountain," as in other passages, is here named as something distinct from either a spring or a well of living waters. But I must content myself with only one more reference. (Hosea xiii. 15.): "Though he be fruitful among his brethren, an east wind shall come up from the wilderness * * * and his spring shall become dry, and his fountain shall be dried up." The word translated "become dry" literally means ashamed; that is, it would become contemptibly small. the other hand, the word translated "dried up" is the same which is applied to the drying up of the waters of the Deluge, and also of the Red Sea. Thus, springs and fountains appear to stand to each other in the relation of cause and effect. When the spring became so small that it was ashamed to show itself, the fountain was dried up as a necessary consequence. But if we give the meaning I have suggested to fountain, then the account of the Deluge would read: *The same day were all the basins of the great deep broken up.*

The next important point is to ascertain what is meant by "broken up." "And the city was *broken up*, and all the men of war fled by night by the way of the gate between two walls." (2 Kings xxv. 4.) "The Lord by wisdom founded the earth, Prepared the heavens by understanding, By his knowledge depths have been '*rent*,' and clouds drop dew." (Prov. iii. 20.) (Young's Trans.)

"And his feet shall stand in that day upon the mount of Olives, which is before Jerusalem on the east, and the mount of Olives shall *cleave* in the midst thereof toward the east and toward the west, and there shall be a very great valley: and half of the mountain shall remove toward the north, and half of it toward the south." (Zech. xiv. 4.

"He putteth forth his hand upon the rock; he overturneth the mountains by the roots. He *cutteth* out rivers among the rocks, and his eye seeth every precious thing." (Job xxviii. 9-10.)

"Then shall the lame man leap as an hart, and the tongue of the dumb sing: for in the wilderness shall waters *break out*, and streams in the desert." (Is. xxxv. 6.) "Then shall thy light *break forth* as the morning." (Is. lviii. 8.)

The Philistines and Arabians "came up into Judah and *brake*" into, and carried away all the substance, &c. Such passages leave no doubt as to the meaning of *baqa*. (1) A city was "broken up;" that is, the wall of the city was broken down. (2) The mount of Olives was to "cleave" in the midst thereof and become a valley; in other words, the crust of the earth was to be disrupted. (3) Light was to "break forth" as the morning: meaning that the ignorance of the world was to be rent asunder, as the rays of the morning sun rend the darkness of the night. (4) Waters were to break out in the wilderness: that is, water was to burst forth at the very place where, according to our experience, we would least expect it. (5) The Lord cutteth out rivers among the rocks. Here, instead of the usual term, the patriarch uses the word *yeor* for rivers, and as yeor is the root of Jordan, we have some grounds for inferring that "cutteth" is applied to that great fissure in the surface of the earth. Thus the original for "broken up" means "disrupted," "cleft," "broken down" (as applied to the defences of a city) or "burst forth." Job uses it in the last sense when he says "Behold my belly is as wine which hath no vent, it is ready to 'burst' like new bottles." But *baqa* also means divided:

> "And Moses stretched out his hand over the sea; and the Lord caused the sea to go back by a strong east wind all that night, and made the sea dry land, and the waters were *divided.*"

The next statement in the narrative that demands our attention is: "And the windows of heaven were opened." Speaking of the Hebrew word *arubbah*, Kirby says: "The radical idea is that of lying-in-wait as a wild beast in its den. In other parts of Scripture it is used for dove cots, or the holes in rocks that doves frequent; for the sockets of the eyes; for the heavens when shedding copiously blessings or plenty; and for the actions of something from above producing earthquakes." The original is thus variously translated, but several of the renderings here given are not free from difficulty. The following passage, however, cannot be misunderstood:

> "Therefore they shall be as the morning cloud, and as the early dew that passeth away, as the chaff that is driven with the whirlwind out of the floor, and as the *smoke out of the chimney.*" "It strikes me," says the author just cited, "as not very improbable that the term I am speaking of may allude to volcanoes and their craters which may be called the chimneys of the globe, by which its subterranean fires communicate with the atmosphere." The expression in Isaiah (chap. xxiv. 18) "The windows from on high are open and the foundations of the earth do shake," seems to indicate that earthquakes are connected with the opening of the windows of heaven, thus pointing to volcanic action as the result."

In consequence of the radiation of heat the nucleus of the earth is slowly contracting, and the solid crust, thereby losing a certain amount of support, settles down; and this adjustment which is constantly going on between the external shell and the shrinking nucleus we call earthquake phenomena. The process is, however, not equable either in time or space; but, like ice on a pond from which the water is being withdrawn, the shrinking goes on by fits and starts, with some of the lines of fracture dipping below the mean level, and others rising above it. In the case of the earth the lower lines of fracture are, to some extent, projected into the molten mass: and the solid rocks exposed, in this way, to the internal fires are reduced to more elementary forms; and volcanoes are simply the chimney by which the lighter products of the combustion reach the surface. But although earthquakes are doubtless the remote cause of volcanoes, yet there is an obvious action and reaction between the two classes of phenomena. The passage in Isaiah, adverted to by Kirby, begins: "Fear, and the pit, and the snare, are upon thee, O inhabitant of the earth." Nothing so produces instantaneous fear and dread as an earthquake. "The impression is not that of simple fear, but a feeling of absolute pain." But the fear is associated with noise, and no other noises are heard at such great distances as the roarings of volcanic mountains. The word here rendered "pit" means an opening. "Open thy doors, O Lebanon, that the fire may devour thy cedars." The use of the word "snare" is very remarkable. We have seen that *arubbah*, translated "windows" in the narrative of the Deluge, is from the root *arabh*, to lie in wait. Now a snare or gin, used figuratively, is also something lying in wait, and is there anything in inanimate nature which can more truly be said to do this than earthquakes and volcanoes. Jeremiah uses the same words. "Fear, and the pit (opening), and the snare;" and as Palestine is full of the remains of earthquakes and volcanoes, may we not almost conclude that it was a proverbial expression for universal and unlimited danger. "In the earthquake," says Humbolt, "direct our flight whithersoever we will, we still feel as if we trod upon the very focus of destruction." "Upon the wicked," says the Psalmist (Psalms xi. 6), "he shall rain snares, fire, and brimstone." The ring of this is as if we heard the cries of Sodom, or saw Vesuvius belching forth the showers of fire, scoriæ, and ashes, that overwhelmed Herculaneum and Pompeii. Using "chimneys" instead of "windows," I will now cite the passage in full (Isaiah xxiv. 18):

"And it shall come to pass, that he who fleeth from the noise of the fear shall fall into the opening; and he that cometh up out of the midst of the opening shall be taken in the snare: *for the chimneys from on high* are open, and the foundations of the earth do shake. The earth is utterly broken down, the earth is clean dissolved, the earth is moved exceedingly. The earth shall reel to and fro like a drunkard, and shall be removed like a cottage."

When Elisha announced incredible plenty to Samaria, "Then a lord, on whose hand the King leaned, answered the man of God, and said, Behold, if the Lord would make *windows* in heaven, might this thing be?" (2 Kings vii. 2.) Here it is not to open, but to make, prepare, or bring forth "windows in heaven," and once formed, it was expected as a possibility, that they would reduce the price of flour and barley *in the gate* of Samaria. The famine had been caused by the siege, and in the hour of extremity it was not by any means a far-fetched idea for unbelief to suggest an eruption of fire and brimstone *as the only way* of getting rid of Ben-hadad and his hosts.

"The grinders cease," says Solomon, "because they are few, and those that look out of the *windows* be darkened." Here the royal poet uses *arubbah* metaphorically for eyelids, and in effect, tells us that the light and fire under them would become dark. The metaphor is frequently associated with lying in wait. "He that winketh with the eye causeth sorrow." * * * * "A violent man enticeth his neighbours, and leadeth him in the way that is not good. *He shutteth his eyes to devise froward things.* * * * * And again, "Lust not after her beauty in thine heart: *neither let her take thee with her eyelids.*"

Isaiah asks? (Isaiah lx. 8) "Who are these that fly like a cloud and as the doves to their *windows*?" Doubtless the Prophet is here referring to the Blue Rock Dove, with respect to which the Rev. Mr Tristam states: "No description can give an idea of the myriads of rock pigeons. In absolute clouds they dashed to and fro in the ravine, whirling round with a rush and a whir that could be felt as a gust of wind." They make their nests "in the sides of the hole's mouth." And as the nests are in the inaccessible cliffs of the mountains, it will be seen that "windows" do not by any means describe the actual situation.

The remaining passage in which *arubbah* is rendered by "windows" Malachi iii. 10. "If I will not open you the *windows* of heaven and pour you out a blessing." The Hebrew word *ruq* is nowhere else translated by "pour out." In six places it is rendered "to draw"—draw the sword; and in other six "to empty," but so far as I am aware in no one instance does it suggest the thought of anything falling from above. Certainly in Eccl. xi. 3 there is the expression, "If the clouds be full of rain they empty themselves upon the earth." But a glance at the context shows that the object of the royal seer was simply to point out the sequence that exists between cause and effect.

If we use empty instead of pour, and chimneys instead of windows, the verse will read: "Bring ye all the tithes into the storehouse, that there may be meat in mine house, and prove me now herewith, saith the Lord of hosts, if I will not open the chimneys of heaven, and empty you out a blessing, that there shall not be room enough to receive it." Here the metaphor, whatever its meaning may be, is used to express the copious character of the blessing, and the blessing itself was to consist of a prosperous husbandry. And before rejecting my suggestion, it will be well to remember that volcanic regions and islands are celebrated for their fertility—Egypt itself deriving all its wealth from volcanic mountains, or perhaps more strictly from what was once the molten tableland of Abyssinia. "In no country in the world," says a writer in Boutell's Bible Dictionary, "is volcanic agency more palpable than in Palestine; in none have earthquakes done their fearful work more frequently or on a more formidable scale; and yet so far as written histories are concerned, nowhere have earthquakes and their consequences been less noticed than in Palestine." This is doubtless true as respects the historical parts of the Scriptures; but, as appears to me, references to them are frequent both in the Psalms and in the Prophets. Amos, in announcing great blessings to the Church, says (Amos ix. 13); "Behold the days come, saith the Lord, that the plowman shall overtake the reaper, and the treader of grapes him that soweth seed; and the mountains shall drop sweet wine, *and all the hills shall melt.*" This is characteristic of a volcanic mountain.

> "Here verdant vines o'erspread Vesuvius' side,
> The generous grape here poured her purple tide."

Martial wrote this eighteen hundred years ago; and at the present day "Vineyards clothe the lower slopes, which, with the dwellings of the peasantry, *are unfailingly renewed* as often as they are ravaged by eruptions, *yielding a delicious wine.*" The prophet Nahum also uses the melting of the hills as a figure: "The mountains quake at him, and the hills melt, and the earth is burned at his presence." Here it is judgment that is spoken of, and the Roman poet did not overlook this vindictive aspect:

"Now flaming embers spread dire waste around,
And gods regret that gods can thus confound."

True the reference to the melting of the hills is figurative, but then the herdsman of Tekoa, who prophesied "*two years before the earthquake*," was too much the son of nature to use a figure not familiar to his countrymen.

The above remarks on the Hebrew original for "windows" may be summed up as follows: First, in one instance the obvious sense requires *arubbah* to be translated chimneys, and it is so translated; in another, the sequence of natural phenomena appears to require the same rendering; thirdly, it is figuratively used for eyelids—something that opens and shuts, combined with the idea of lying in wait; fourthly, it is an appellation given to caves or openings in mountain cliffs, the retreat of clouds of rock doves; fifthly, the opening of it was to have the effect of causing cheap food in the gate of a besieged city; and lastly, to bring the blessing of a prosperous husbandry. In all this the idea of "windows" never appears to enter; whilst in two, at least, of the instances the sense requires "chimneys."

Another thing well worthy of attention in the account of the Deluge is that three Hebrew words, namely, *tsohar*, *arubbah*, and *challon* are all translated window or windows. The first means "brightness" or transparent thing. "A window *(tsohar)* shalt thou make to the ark, and in a cubit shalt thou finish it above." We would probably express the same thing by saying, "Glass of given dimensions shalt thou put in the roof." When, however, we want more light and air we do not say "open the glass," but "open the windows." And we find exactly the same sequence in the Bible narrative: For when the waters abated, "Noah opened the window *(challon)* of the ark, which he had made." And to say the least of it, it is a striking fact that this is the first time *challon*, the proper original for window, is used.

For the above reasons I think Kirby's suggestion ought to be adopted, making the passage describing the Deluge read:

"*The same day were all the basins of the great deep disrupted and the chimneys of heaven* (volcanoes) *were opened.* And the rain was upon the earth forty days and forty nights."

Now compare this with the retreat of the water: "And God made a wind to pass over the earth, and the waters assuaged. The basins also of the deep and the chimneys of heaven were stopped, and the rain from heaven was restrained."

It will be observed that in the first passage the disruption of the solid crust of the earth is followed by the eruption of the volcanoes, and both are succeeded by rain—now natural rain; and in the second, there is the same order in narrating the facts, the only difference being that a great storm preceded the cessation of the other phenomena. This storm is deserving of attention. The great waves of translation, which my theory presupposes, carried the ice and ice-cold water of the Arctic and Antarctic circles into low latitudes; and this must have produced an almost uniform temperature from pole to pole. Now the capacity of the atmosphere for containing aqueous vapour rises or falls in sympathy with the temperature, and hence the cooling of the air in all temperate and tropical climates necessarily produced such a rain as the world had never seen. As is well known, aqueous vapour enters as an important element into all the habitudes of the atmosphere, affecting its height, gravity, volume, and mobility; and consequently its abstraction in such enormous quantities must have destroyed the "circuit of the winds." The effect on the equilibrium would, however, be most felt when the re-adjusting process set in; for the vapour then added to the atmosphere between the tropics would not be compensated for by a corresponding addition in high latitudes, and the consequence would be a storm from each pole towards the equator with a superior current in the opposite direction.

Such great oceanic movements and high winds throw light on the extreme caution of Noah in leaving the Ark. He and his family had seen the waters of the ocean "go up by the mountains and down by the valleys," and they had also felt the very foundations of the earth stagger under them. But even the dying throes of the great cataclysm were calculated to suggest caution for it is stated, "Anc they restored themselves as formerly, the waters from over the eartl by the going off and the coming back." It was seventy-three days after the Ark grounded before the tops of the nearest hills were seen, and yet Noah did not venture to open the window until the end of other forty days. When in the end he did take steps his proceedings were of a very tentative character. He first sent out the raven, and when it did not return, he at the end of seven days sent out the more delicate dove. But although the dove proved a faithful messenger and brought back to him the proof that he wanted, yet Noah remained unsatisfied with the evidence. And the fact that he did so looks very like unbelief. Anyway the dove at the end of other seven days was despatched a second time, but returned no more. In whatever way its non-appearance was interpreted other seven days passed in inaction. The removal of the cover was followed by a further stay of two months and seventeen days; and when the landing finally took place it was at the express command of God.

These views also explain why the rainbow was adopted as the sign of the covenant. The great waves of translation, necessarily produced at all parallels of latitude and at all depths the same uniformly low temperature, and blocked up the channels by which the dry land had been previously drained. This obstruction to the drainage covered the surface with lakes and swamps; and the evaporation from these, as well as from the earth, everywhere saturated with water, cooled down the air and destroyed all the adjustments that usually bring fair weather. All this is obvious enough, and any one that has given attention to meteorology will see that under the circumstances sunshine was impossible. Noah and his family had suffered an imprisonment of about a year, and during a great part of this time they had been driven hither and thither apparently the sport of a dark and stormy ocean. They had, it is true, felt the Ark grounding, and doubtless they were to some extent cheered by the gradual emergence of the land; but such encouraging circumstances were counter-balanced by the knowledge that instead of the blue vault of heaven they were enveloped in a swaddling band of thick darkness—of clouds, fogs, and drizzling rain. None knew better than the survivors of the Deluge how essential sunshine was to their very existence, not to speak of comfort; and hence what could have been better fitted to restore their confidence than the announcement: "My bow I have given out in the cloud, and it hath become the token of a covenant between me and the earth." In other words, God caused the long hidden solar rays to break through the universal gloom producing the old familiar rainbow. And the opportune appearance of the beautiful phenomenon was not only to be accepted as a token of God's immediate favour; but it was thenceforward to be looked upon as an assurance that the waters of the Deluge would no more cover the earth.

The long absence of the sun also probably explains why Sabianism or sun-worship was the first form of idolatry. It has been well said that we never know the value of our greatest blessings till we lose them. The antediluvians doubtless believed in the immobility of the solid sphere, in the permanency of the then existing relations between the sea and the dry land, and above all, they had faith in the daily rising of the sun. Of these three articles of faith the shock of an hour shivered to atoms the two first; and a year's experience must have taught Noah and his family the delusiveness of the last. At any rate, from the moment the happy advent of the rainbow gladdened the hearts of these tanned children of the East, the sun, moon, and stars would be viewed with a love and an interest far exceeding anything in antediluvian times. And if we keep this in mind it is easy to see how, when, after the division of the earth, ignorance and philosophy came to supplant the simple sacrifice and piety of Noah, the sun, as the giver of light and heat, became an object of reverence and in the end took its place next to the hidden one. Curiously enough, on the more ancient hieroglyphic monuments of Egypt the principal deity was styled "Amun," "the hidden unrevealed one;" which to this day, is, in Arabic, one of the names of the true God. Now it is an instructive fact that after the 18th dynasty, "Ra," or the sun, was in the inscriptions affixed to Amun, converting the name into "Amun-Ra." The family of Noah had of course a sad tale to tell to their children, but so far as it was personal to themselves it was a tale of gratulation and thankfulness. Judging from analogous cases the narrative reached the grandchildren with some of the features exaggerated, and others toned down; and with every further remove, the parts played by the individual survivors of the Deluge would come more largely into the foreground. Poets and fabulists probably contributed their quota of fanciful details, and with so much material ready at hand the reverence of later generations could readily exalt into more than heroes ancestors whom Providence had so signally favoured. There is possibly not much in it; but it is at least a singular coincidence that the Egyptian gods worshipped at Memphis differed from those at Thebes, and yet the first order at each place contained eight, just the number of persons in the Ark. Equally significant is the case of Phœnicia which next to Egypt was one of the oldest of states. In addition to Baalsamin and local deities there were eight worshipped by all Phœnicians. "They were called the children of Sadik (the Just), or the Pataeki (Descendants of Phtha), or the eight Kabiri (Strong Ones). They were the maritime gods, and their images were placed on the prows of Phœnician ships." (Chamb. Ency.)

The bearing of these views on the distribution of Man and on the languages of the world is very obvious. At the time the waters of the Deluge came to rest, the Icy Sea, as we have already seen, probably reached the foot of the Altai Mountains; and the respective basins of the Caspian, the Aral, and the other great concavities without outlets in the Eastern plateau were all full of water to the brim. This restricted the dry-land area: but as a set off, the additional moisture and warmth in winter must have given to what remained an Italian climate. And when we place along side of this the obvious fact that so long as Noah lived the influence of his age and character must have secured good government, we may venture to conclude that ancient Bactria was as near a second Paradise as anything we can look for in this world. And this obvious connection between the Deluge and peace receives striking support from the researches of Max Müller.

"It should be observed," he states, "that most of the terms connected with the chase and warfare differ in each of the Aryan dialects, while words connected with more peaceful occupations belong generally to the common heirloom of the Aryan language. The proper appreciation of this fact will show that all the Aryan nations *had led a long life of peace* before they separated, and that their language acquired individuality and nationality as each colony started in search of new homes—new generations forming new terms connected with the warlike aud adventurous life of their onward migrations. Hence it is that not only Greek and Latin, but all Aryan languages have their peaceful words in common; and hence it is that they all differ so strangely in their common warlike expressions. Thus the domestic nimals are generally known by the same name in England and in

India, while the wild beasts have different names, even in Greek and Latin."

SEC. XIX.—THE COSMICAL CHANGE OF LEVEL THAT FOLLOWED THE DELUGE.

NORTH ATLANTIC.	SOUTH ATLANTIC.	SOUTH PACIFIC.	NORTH PACIFIC.
a. Gradual Depression.	*b.* Gradual Elevation.	*c.* Gradual Depression.	*d.* Gradual Elevation.

As pointed out in Sec. 14 the Diluvial sea came to rest in something like the following position: The British Isles and New Zealand respectively stood from 700 to 1000 feet higher, and the two reciprocal quarters were correspondingly depressed, the Ocean covering the Pampas of S. America in the one, and all the flat Tundras of Eastern Siberia in the other. The elevation of the land in the N. Atlantic and S. Pacific brought on a second glacial period over each of these quarters of the sphere, and the glacial conditions again brought the same forces into play that produced the great Tertiary shifting of the axis of rotation (see sec. 7). It has been remarked that the sea stood from 700 to 1000 feet lower round the British Isles: but American geologists have concluded that it also stood about 450 feet lower in the Gulf of Mexico; and these figures indicate that the great post-tertiary oscillation had its meridian of maximum effect near 30° W. of Greenwich. From this line the vertical amount necessarily decreased on both sides till it vanished altogether at 60° E, and 120° W.; and considered from a north and south point of view the vertical amount of the oscillation would increase from the equator to about the parallels of 60°. Add to this that Greenwich and New Zealand were respectively some 10° or 15° further from the equator, and we will, as I believe, have a tolerably fair idea of the actual state of the case at the beginning of Post-diluvial times.

a. As regards the N. Atlantic basin I must for the sake of clearness recapitulate some things already advanced; and I will begin with the eastern border of N. America. During what is known as the Champlain period the sea rose in the Gulf States from being 450 below its present level to about 400 feet above it. On Lake Champlain the elevation above the present shore line must have been about 460 feet; at Montreal 450 above the St Lawrence; and at Barrow's Strait, the height is said to be about 1000 feet.

Passing now to the western border of Europe: R. A. C. Austen, Esq., has proved that in the south of England marine deposits of post-tertiary times overlie sub-aerial beds of detritus which again rest on marine strata said to be of the age of the Crag; and as the sub-aerial beds are charged with angular stones, Mr Austen has concluded that the last layer was formed when the land stood much higher out of the sea than at the present day. The meaning of this appears to be that the Pliocene Sea retreated from the spot to a much lower level; the elevation of the land brought on conditions severe enough to disintegrate the rocks; and after the layer of angular detritus was deposited the sea rose once more and covered it with a marine formation. On the East coast of England Messrs Wood and Rome have proved that what they call the Hessele clay "has a general uniform thickness varying from 10 to about 20 feet, and may be said to wrap the whole of Holderness and East Lincolnshire like a cloth, spreading over and enveloping *the denuded edges* of the basement and purple clays." The Hessele clay rises to an elevation "approaching 150 and perhaps even 200 feet," and it is important that it was deposited "*posterior to the formation* of the valley system of denudations in the interval that followed *the elevation of the glacial beds.*" It will be observed that the Hessele clay is at an elevation of from 150 to 200 feet, but clays are never deposited except in water of some depth, and hence it may be concluded that the high water line stood above these figures. The importance of the evidence is that the valley system, cut into the glacial beds, represents Mr Austen's sub-aerial beds, and both proved an elevation of the land followed by a depression.

As regards Ireland Mr Kinahan states that "in many places below the 350 feet contour line, and in all places below the 250 contour line, the Glacial drift, whether Boulder-clay drift or *Moranic* drift, is more or less washed and sorted into silt, sand, gravel, and shingle." The *Moranic drift* shows that, like England, Ireland had been sufficiently elevated not only to produce severe conditions but that the glaciers descended to what are now low levels; and further the glacial conditions were followed by a rise of the sea to at least 250 feet above the present high water mark. It is well known that what are called the Clyde beds of Scotland overlie the denuded surface of the Boulder-clay. It is also well known that marine shells belonging to the latter formation are found in localities widely remote from each other and at considerable elevations above the sea. These shells, it will be remembered, are different from those that inhabited the same shores in glacial times, and Scotch geologists have failed in filling up the gap—the most obvious explanation being that during the post-tertiary depression the boreal species sought their natural habitat farther north. From the evidence furnished by caves in the Bay of Palermo, the late Dr Falconer concluded "that the Maccagnone Cave was filled up to the roof within the human period, so that a thick layer of bone-splinters, teeth, land-shells, hyænas' coprolites, and remains of man was agglutinated to the roof by the infiltration of water holding lime in solution. That subsequently, and within the human period, such a great amount of change took place in the physical configuration of the district as to have caused the caves to have been *washed out and emptied of their contents*, excepting the patches of material cemented to the roof and since coated with additional stalagmite." But to wash out the contents of this cavern required a depression of the land of about 250 feet.

Crossing to Northern Africa, the Great Sahara, according to M. Martins, is nothing but a dried-up sea bottom, and the fossil shells are of the same species that now inhabit the Mediterranean. But the retreat of the sea in the Mediterranean was noticed by the poet Pindar, five centuries B. C. Speaking of the island of Delos he writes, "Hail ocean's youngest child." Both Rhodes and Delos were, according to Philo, "overwhelmed by and sunk under the sea: but by *lapse of time*, as the sea gradually diminished, they by *slow degrees rose above it and came into sight*, as the histories which are written concerning them record." On the shores of the Dardanelles, says Captain Spratt, the height of the marine deposits indicates a former depression of about 60 feet. And it is not to be forgotten that the depression of the land round the basin of the Mediterranean followed the great elevation that produced a second great extension of glaciers on the Alps and on the Atlas Mountains, and which also had the effect of making the Black Sea fresh. Manifestly the fall of the level of the Atlantic of from 700 to a 1000 feet below the present shore line, as proved by the littoral species of shells dredged by Mr Gwyn Jeffreys from the bed of the German Ocean, would cause the waters of the Mediterranean to rush out at the Straits of Gibraltar; and the consequent fall of the level would again determine a strong current from the Euxine; and hence the freshness of the latter sea.

"We find recorded in Strabo," says the Rev. Dr Goodenough, "the opinion of Straton of Lampsacus; that the Euxine was very shallow; that it was then filling up with mud from the deposits of the rivers; and that its water was perfectly fresh." (Geog. Jour. vol. 1.)

Many years ago Leop. Von Buch called attention to the fact that in the neighbourhood of Fehrbellin, twenty-three miles from Berlin, *marine plants* are not unfrequently found in the mosses. Again Prof. Kjerulf gives a confident opinion that since the Glacial Period the sea has stood in Scandinavia at an elevation of 600 feet (and no more) above its present level. To these proofs may be added a fact or two from the eastern edge of our quarter of the sphere. "Dr Malcolmson informs me," says Darwin, "that he collected shells and corals, apparently recent, from the cliffs of Camaran Island (Red Sea, lat. 15° 30′) at a height of between 30 to 40 feet"; and Mr Salt describes a similar formation a little southward on the opposite shore at Amphila. Moreover near the mouth of the Gulf of Suez, although on the coast opposite to that on which Dr Rüppell says that the modern beds attain a height of only 30 to 40 feet, Mr Burton found a deposit replete with existing species of shells at the height of 200 feet. (Coral Reefs.)

In the Geol. Jour. vol 19, there is a very instructive notice of a Memoir by Dr Szabó of Pesth. On the plains adjacent to the Danube and other European rivers that fall into the Black Sea, there is an extensive alluvial deposit superimposed on the true Loess; and from the height of the older inundational areas covered by this alluvium, Dr Szabó has drawn the following conclusions:—

"That the land must have risen from 100 to 130 feet since the deposition of the old alluvial deposits at Semlin, Gomba, Kis-Terenne, &c.

And that there has been an upheaval of from 130 to 200 feet since the terrace-system was formed; these terraces, however, are not the result of the action of rivers, but of that of a vast expanse of water, which covered the whole area of the diluvial deposits."

Manifestly, when the flow of the waters through the Straits of Gibraltar became inwards instead of outwards the effect would be to cause the land round both the Mediterranean and Black Seas to sink; and we have just seen what was the vertical amount of the depression. But the level of the Black Sea could not have risen say 130 feet without reducing the fall of all the *European rivers* flowing into it. In its course from Vienna to the sea the Danube runs east over thirteen degrees of long. and north through three degrees of lat. This in the case of such a change of level as I am discussing would probably reduce the height of Vienna above the sea from 420 to 300 feet, or about one-fourth of the whole fall of the river. This would reduce its sediment-carrying power and the force of the current. In such altered conditions, the river, in a greater or less degree, must have filled up its own channel, and inundated the neighbouring plain with its now sluggish current. This, as appears to me, is the true explanation of the inundational areas of Dr Szabo's Memoir. It can hardly be necessary to remark that in studying the effect of a cosmical change of level on the fall of a river, the essential point is the direction of its course in relation to the lines of maximum and minimum effect. For instance the rise that reduced the fall of the Danube necessarily increased that of the Rhine; and it is interesting to find that the alluvial plains of the Danube have their exact counterpart in N. America. The Mississippi flows south through eighteen degrees of latitude, and for the first 850 miles from its mouth the fall does not exceed five inches per mile; and from figures and circumstances given by Dana there are grounds for believing that on the north side of Lake Superior the Post-tertiary depression could not have been less than 930 feet. In other words, the sea rose at Lake Superior 480 feet higher than at New Orleans—a vertical depth equal to the whole fall of the Mississippi for the first 1100 miles. As in the case of the Danube this gradually reduced the force and velocity of the river; and a partially filled up channel and a sluggish current, caused an inundation of all the low-lying ground near the banks. And it is the silt deposited at the time in question which forms the loess of the American geologists, and it occupies the same zone as the

Clyde beds in Scotland, the Hessele clay of Messrs Wood & Rome, the fossil algae in the peat bogs near Berlin, and the elevated "inundational areas" which formed the subject of Dr Szabo's researches.

b. In his Geological observations on S. America, Darwin has abundantly proved a great Post-tertiary elevation. Speaking of the Atlantic side of the continent he says: Upraised shells, of species still existing as the commonest kinds in the adjoining sea, occur, as we have seen, at heights of between a few feet and 410 feet, at intervals from latitude 33° 40′ to 53° 20′ S. * * * * * It may be concluded that the coast on this side of the continent, for a space of *at least 1180 miles has been elevated* to a height of 100 feet in La Plata and of 400 feet in Southern Patagonia within the period of existing shells, but not of existing mammifers. It is well worthy of notice here that the height of the shells increases in going southward. With respect to the West Coast of S. America, he says: "We have seen that upraised marine remains occur at intervals, and in some parts almost continuously, from lat. 45° 35′ to 12° S. along the shores of the Pacific. This is a distance, in a north and south line, of 2075 geographical miles * * * * Judging from the upraised shells alone, the elevation in Chiloe has been 350 feet; at Concepcion certainly 625, and by estimation 1000 feet; at Valparaiso 1300 feet; at Coquimbo 252 feet; northward of this place, sea shells have not, I believe, been found above 300 feet; and at Lima they were falling into decay at eighty-five feet." * * * * *

"Finally, comparing the extent of the elevated area as deduced from the upraised recent organic remains, on the two sides of the continent, we have seen that on the Atlantic, shells have been found at intervals

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In a previous section I drew the inference that Darwin did not give due significance to the gap in time that intervened between the early tertiary formation and the overlying Pampas; however, I now find that I had overlooked a section of a cliff at Port S. Julian, which shows that a stream had cut its channel through the tertiary strata before the post-tertiary formation with its remains of the Macrauchene were deposited. I had also overlooked the following observation: "We have further seen that in this district, at a period *not only subsequent* to the deposition of the tertiary strata, *but to their upheavement and most extensive denudation,* true Pampean mud with its usual characters, and including mammiferous remains, was deposited round and between the hills or islets formed by these tertiary strata, and over the whole eastern and low primary district of Banda Oriental."

from eastern Tierra del Fuego for 1180 miles northward, and on the Pacific for a space of 2075 miles. For a length of 775 miles, they occur in the same latitudes on *both sides* of the continent * * * * * From the continent being narrow in the southernmost parts of Patagonia, and from the shells found at the Inner Narrows of the Strait of Magellan, and likewise far up the valley of the S. Cruz, it is probable that the southern part of the western coast, which was not visited by me, has been elevated within the period of recent mollusca; if so, the shores of the Pacific have been continuously, recently, and in a geological sense synchronously upraised, from Lima for a length of 2480 miles southward—a distance equal to that from the Red Sea to the North Cape of Scandinavia." (Geol. Obsers. on S. America.)

It will be remembered that S. Africa, like S. America, has also been elevated, and that the Post-tertiary deposits fringe the coast, rising at Delagoa Bay, according to Mr Stow, to at least 100 feet. It will also be remembered that the fossil shells belong to species now living near to the equator. In the Mauritius Darwin observed proof of an elevation of 30 feet. "The northern extremity of Madagascar is," he states, "described by Captain Owen as formed of madreporitic rock, as likewise are the shores and outlying islands along an immense space of Eastern Africa, from a little north of the equator for 900 miles *southward.* Nothing can be more vague than the expression "madreporitic rock;" but at the same time it is, I think, scarcely possible to look at the chart of the linear islets running in front of the coast from the equator far southward, and rising to a greater height than can be accounted for by the growth of coral, without feeling convinced that a line of fringing reefs has been elevated at a period so recent, that no great changes have since taken place on the surface of this part of the globe."

c. On my hypothesis the Post-tertiary sea rose the same vertical distance in the S. Pacific as in the N. Atlantic; but I have no direct proof of the fact, such as that furnished by the littoral species of fossil shells now at a depth of 600 or 700 feet under the German Ocean, or of the proof of a rise of 450 feet furnished by the sinking of a deep well at New Orleans; and hence I have to fall back on indirect evidence. I have already contended that the Post-tertiary extension of glaciers in New Zealand can only be explained on the supposition of a greater elevation of the land; and the form in which the coral islands and coral reefs have been built up affords convincing proof not only of the elevation but of the subsequent depression.

I have already more than once had occasion to remark that the direction of a change of level due to a shifting of the axis of rotation is at every step of its progress determined by the conditions existing for the time being; and hence the improbability of a great oscillation moving throughout its whole range between the same meridian lines. For instance it cannot be supposed that the accumulation of snow and ice on the one hand and the drying up of inland seas on the other proceeded uniformly either as regards time or space. Moreover, owing to the unequal distribution of the dry land, large areas would emerge in one quarter of the sphere, and similar areas would be submerged in others, and all such changes would be so many new factors imported into the problem. Still, notwithstanding these disturbing causes, it is pretty clear that the direction of the oscillation did not shift much; and the meridian lines of 60° E. and 120° W. will in an off-and-on way separate the areas of subsidence from those of elevation. Well then taking these two meridians as dividing lines, we have a quarter of the sphere which, with a few trifling exceptions, includes all the atolls and barrier reefs in Dr Darwin's chart; and even the exceptions are of that class which, instead of invalidating the rule, afford it support.

Ocean Island, for instance, in lat. 28° 22′ N. and long. 178° 30′ W. is coloured as an atoll on the map, but the author (Darwin) frankly admits that considerable doubts "should be entertained regarding the nature of a reef of this kind with a very shallow lagoon, and standing far from any other atoll, on account of the possibility of a crater or flat bank of rock lying at the proper depth beneath the surface of the water, having afforded a foundation for a ring formed coral reef." Few islets having been formed on the reef, and the lagoon being shallow, there is clearly a want of evidence either of depression or elevation; and Dana does not appeared satisfied that it is coral rock at all.

Pratas Shoal, lat. 20° N.:—Citing Horsburgh, Darwin states that this shoal is composed of coral, and has a low islet upon it; but as there are places outside the breakers where a ship can find anchorage, it is clear that the question of a depression is extremely doubtful.

Dana dismisses this shoal with the general remark: "In the China Seas there are numerous shoals, banks, and island reefs of coral."

The Paracells:—This group is also coloured as atolls, but they do not appear to have much claim to the distinction; for we are told that "only a few islets have been formed on *these shoals, and this seems* to be a general circumstance in the China Sea."

Sahia de Malha Bank:—These banks are classed as atolls. They are nearly on one of the meridians which I have assumed as the line of no change; and as they are described as a series of narrow banks with from eight to sixteen fathoms of water on them, a depth at which corals can live, they furnish no evidence one way or other.

In the Mozambique Channel, Dr Darwin has, on the authority of others, classed the islands of Cosmoledo as an atoll, and two or three of the Comoro group as belonging to the barrier class; but as the opposite shores of Madagascar on the one side, and Africa on the other, as well as the Seychelles on the north-east, are all surrounded by fringing-reefs, it is manifest that if the facts afford any evidence of a depression at all, it could only have had a local range. The above are all the atolls and barrier reefs outside of what I have called the S. Pacific quarter of the sphere except the Laccadives and the Maldives. However, we must not forget that on my hypothesis it is here assumed that the equator on the meridian of 150° E. was at the beginning of the Post-tertiary era at least fifteen degrees farther north than now. The Laccadives I must dismiss with the remark that I have not been able to get much information about them. They are said to be based on coral reefs and nearly flat; but as Andernot, the largest of the group, is said to be three miles long and *one broad* it cannot possess much of the atoll character. Indeed Lieutenant Wood states "it not only presents a bold front to windward but *that front* is one side of the island itself and not a reef as is generally the case." * * * * "In the others without a single exception, the reefs are situated to windward of the islands." Although Darwin has coloured the Laccadives as atolls, yet it must have been with hesitation, for he adds the qualifying remark: "The atoll-like structure is less evident than in the Maldives." The last named group is very interesting. The islands extend in a north and south direction for 470 miles, and at the beginning of our era the equator must have cut the line in two and hence on my view of an elevation on the one side, and a depression on the other, we ought to expect some characteristic distinctions between the north and south atolls of the group. And such indeed we find. "The lagoons of all the atolls in the southern part of the Archipelago are," says Darwin, "from ten to twenty fathoms deeper than those in the northern part. * * * * * I can assign no adequate cause for this difference in depth excepting that the southern part of the Archipelago has subsided to a greater degree or at a quicker rate than the northern part." * * * * * "I am informed by Captain Moresby, the westerly winds are the strongest, yet the islets are almost all placed on the eastern side of the northern atolls, and on the south-eastern side of the southern atolls." * * * * * In the three southern large atolls the separate portions of the reef between these channels have the ordinary structure and are linear; but in the other atolls, especially the northern ones, these portions are ring-formed like miniature atolls." * * * * * In the southern atolls, of which there are nine large ones, all the small reefs within the lagoons come to the surface, and are dry at low water spring tides. This circumstance is very remarkable as within some atolls not a single reef comes to the surface."

The above extracts show how in almost every feature the northern atolls are unlike the southern ones; and as the islands lie on the meridian, and must have been bisected by the equator we have just such a difference between the two ends of the chain as might have been expected from a shifting of the axis. In the next section I shall have occasion to revert again to the outlying coral reefs: that is reefs north of my hypothetical equator and not within 60° E. and 120° W.: and I will here invite attention only to the very important fact that none of the outliers are typical specimens either of atolls or of barrier reefs. True it is not easy to account in all cases for the peculiarities wherein they differ from those of the Coral Sea, but the undoubted fact that they do differ affords good grounds for believing that they did not grow up simultaneously under the same set of conditions.

Leaving out of account questions of elevation and depression, there does not appear to have been any important change in the outline and contour of the continent of Australia since its emergence from the Tertiary Sea, and perhaps even longer; and the fact has an important bearing on my argument. All the way from Torres Straits to Sandy Cape, extending through fifteen degrees of latitude, or about 1000 miles, the East coast is fringed by the well-known great Barrier Reef. It begins at the southern edge of the Coral Sea and runs north beyond Cape York; and taking the length and direction of the reef in connection with the permanence of the coast line, it is clear we must study it as a whole; and when we do this we find two facts, the one complemental to the other, which make it quite evident that the change of level could not have been merely local. Competent surveys have proved that both the distance between the reef and the shore, and the soundings increase in going south. Again, as has already been more than once pointed out, in the case of a change of level due to the shifting of the axis the vertical amount increases with the latitude, and this is just what has taken place along the whole Barrier Reef of Australia. Further the reef that skirts the west side of New Caledonia, although it has more east and west in its direction, and is only 400 miles long, yet so far as it goes, the evidence which it affords is in strict accordance with that of the Australian reef; for we are told that for many leagues it is only eight miles from the shore, but near *the southern end* of the island the distance increases to sixteen miles.

I have said that round the British Isles, the level of the sea was probably from 700 to 1000 feet below the existing shore line; but doubtless the vertical amount was greater on the meridian of 30° W.; say from 1200 to 1500 feet. Now let us compare these figures with the depth of the lagoon channel within the Barrier Reef of Australia. In going south the soundings, we are told, increase from twenty-five to forty, and even sixty fathoms. This does not of course show the whole depth, as some allowance must be made for the filling up of the channel with coral sand. Still if the depression of 25 fathoms at Torres Straits, and 60 at Sandy Cape, kept on increasing in the same ratio, we may conclude that at 55° S. it would amount to at least 1000 feet, which would be a fair agreement with what I have suggested for the N. Atlantic.

Some writers indulge in language, that to me, at least, appears extravagant, as to the time it must have taken to build up the reefs and atolls, and it will therefore be advisable to recal some of the conditions which cannot be left out of account without the risk of serious error. By earthquakes sedimentary matter is projected down into the seething mass, where it is reduced to its original elements; and by volcanoes the gases and lighter products of this subterranean combustion reach the surface; but the time when this takes place is determined by the superincumbent pressure on the one hand, and the tension or elastic force acting from below on the other. Another obvious circumstance that affects this class of phenomena is that the solid crust is necessarily thinnest where the bed of the Ocean is deepest; and hence there is the antecedent probability that fresh volcanoes will break out more frequently at the bottom of deep seas than on dry land. True, in its ordinary quiescent state, the equableness of deep-sea pressure acts as an equipoise to the relative thinness of the crust; but at such a turbulent crisis as ushered in the Post-diluvial era, manifestly all the chances would be on the other side. Because the wide and comprehensive disruptions, and the great upheavals here and the engulfments there, destroyed the previously existing equilibrium between the solid crust and the nucleus, the great engulfments again carried down an unlimited supply of fuel to the interior fires, and when we add that the great cosmical change in the level of the Ocean had removed a pressure of some 2000 or 3000 feet from part of the S. Pacific, we have just such a set of conditions as must have opened the chimneys of heaven with unexampled intensity. In this way submarine volcanoes would be thrown up, and this more especially along any weak lines of fracture where the subsidence of the Ocean was greatest. The same subsidence would probably bring many old extinct craters near to the surface, as well as the tops of submarine mountains and even long drowned coral islands that belonged to a previous era. All these, along with the new craters, would form so many unoccupied foundations on which the coral architects could begin their labours. That some of the atolls at least are built upon volcanic cones cannot, I think, be gainsaid; because round many of them the soundings show that the descent is by a succession of ledges or terraces, and this is very much what might have been expected on the supposition that the polyps found in submarine volcanic mountains, foundations for their wonderful erections. On land the ejected ashes and pumice conceal to a great extent the successive stages by which the cones have been built up; but at sea all the lighter matter is washed away, leaving only the step-like layers of solid lava. Again some atolls have internal ledges, and this also is a known characteristic of some volcanoes.

Doubtless many of the volcanoes long remained active, and, if we suppose the mass of erupted matter to have accumulated at a greater rate than the general depression of the region, the coral rocks formed round such islands would evidently be of the fringing class of reefs. We appear to have a good illustration of this in the Navigator's group of nine islands. With one exception, they are all of volcanic origin, and in his map Darwin has coloured them all as belonging to the fringing class. The exception is Rose Island; but indeed it is no exception at all, for manifestly it has a volcanic foundation, and Mr Couthouy is cited to the effect: "It consists of a reef, only a league in circuit, surrounded by a very few low islets; the lagoon is very shallow, *and is strewed with numerous large boulders of volcanic rock.*" On this question of the volcanic basis of many coral islands our author cites two very significant facts with respect to the Cook and Austral Islands. Of the six, "five were described to me by the Rev. J. Williams as formed of coral-rock (associated with some basalt in Mangaia), and the sixth as lofty and basaltic." Friendly Islands: This group is coloured as fringed, and it is said that the great majority of the islands are formed of coral; still a number of them are unquestionably of volcanic origin; and in Toufoa and Amargura volcanoes are still active. But with respect to the connection between many coral islands and volcanoes I cannot do better than cite Dana, who has enjoyed so many opportunities of studying the subject:

> "Volcanoes, either extinct or active, constitute *half of the islands* in the Pacific ocean. * * * * * Volcanoes occur in oceanic islands, nearly all of which, excepting a few of very large size and the coral islands, are throughout volcanic; and the coral islands have probably a volcanic basis. * * * * * At the Hawaian Islands there are the remains of ten or more volcanic mountains, and two on Hawaii are now active. * * * * * There are other volcanic mountains at the Society Islands, Marquesas, Navigators, Friendly Islands, Fejees, Santa Cruz group, New Hebrides, Ladrones; among which Tanna and Ambrym in the New Hebrides, Toufoa and Amargura in the Friendly group, Tinakora in the Santa Cruz group, and two or three in the Ladrones are in action."

Such facts are of great importance; because if we conclude that the majority of coral islands have a volcanic basis, then in that case the mere depth of soundings round their outer edge affords no proof as to the time required to build up the atoll, and for the obvious reason that soundings taken in the same way round a drowned volcanic mountain would give much the same result. Neither does the mere mass of coral rock by any means afford trustworthy data for measuring time. Coral-polypifers are reproduced both by ova and by budding, and carbonate of lime being the food or pabulum on which they live and rear their wonderful structures, the supply of this substance appears to be the only restriction placed by nature on the multiplication of individual forms. The most carefully conducted analysis detects only a trace of lime in the existing seas; but it is easy to see that the great convulsions and waves of translation that have passed under our consideration must have supplied this element in excessive quantities. The ocean must have been stirred up to its profoundest depths, and the diluvium which it deposited on land was everywhere of a highly calcareous nature. And hence for a very long time every river and stream must have reached the sea charged with lime. Again, in the great volcanic activity we have another obvious source to which we can refer for further abnormal supplies. Moreover, in hazarding any conjecture as to the quantities likely to be derived from this source, it is not to be forgotten that the known atolls and volcanic islands of the Pacific probably do not amount to a tithe of the thousands of cones that stud its unfathomable depths. Neither is it to be forgotten that the great majority of the islands at or above the surface are within that area where the diluvial upheaval was on my view the greatest; and it is to this fact and to the industry of the reef building corals that we are indebted for our knowledge of their very existence. Therefore we may, I think, reasonably conclude that the volcanic action of the time is known to us only by comparatively few specimens and that the great majority of the cones are at depths below the zone of coral life. But whatever may have been the depth, it may be taken for granted that at every succeeding eruption each volcano would vomit forth lime; and if, as I have conjectured, the volcanic area extended towards the American continent, then, in that case, the currents of the Pacific flow in the right direction to transport the lime to the Coral Sea. And this is not all. The same causes that produced the carbonate of lime must also have produced carbonic acid, and consequently the sea-water must have been charged with an excess of it. But carbonic acid gas is a solvent of carbonate of lime, and for this reason the capacity of the sea-water for holding lime in solution must have been largely increased.

Moreover, no adequate conception of the post-diluvial development of corals can be obtained unless we take into account the fact that the Deluge necessarily disturbed the whole of the adjustments by which the excessive multiplication of some species is kept in check by the predatory habits of others. That this is not an unimportant consideration will be seen by the following extract:

"There are," says Darwin, speaking of keeling atoll, "other and unexpected agents at work here; large shoals of two species of Scarus, one inhabiting the surf outside the reef and the other the lagoon, subsist entirely, as I am assured by Mr Liesk, by browsing on the living polypifers. I opened several of these fish, which are very numerous and of considerable size, and I found their intestines distended by small pieces of coral, and finely ground calcareous matter." * * * * * "Dr J. Allan of Forres informs me that the Holuthuriae (a family of Radiata), subsist on living coral. * * * * * The number of the species of Holuthuriæ, and of the individuals which swarm on every part of these coral reefs, is extraordinarily great; and many ship-loads are, as is well known, annually freighted for China with trepang, which is a species of this genus."

According to Agassiz corals suffer greatly from innumerable boring animals: "On the loose basis of a Mæandrina, measuring less than two feet in diameter, we have counted not less than fifty holes of the date-fish—some large enough to admit a finger—besides hundreds of small ones made by worms. But however efficient these boring animals may be in preparing the coral stems for decay, there is yet another agent, perhaps still more destructive. We allude to the minute boring-sponges, which penetrate them in all directions, until they appear at last completely rotten through." [Oana.]

Here we have examples of the existing checks to the growth of coral reefs; and a moment's reflection must satisfy us that the violence and comprehensive character of the diluvial oscillation necessarily suspended all such living checks; and that a long time probably elapsed before the same species became re-established in their old habitats. The question of living checks has another element of uncertainty; although, in the Pacific, the post-diluvial depression was, on my view, a gradual one, yet it was not by any means a comparatively slow change of level: and hence, the polyps may have only kept pace with the rising level of the sea without ever actually reaching the surface: and manifestly such conditions must have been in every way favourable to the upward growth of the reef, whilst on the other hand the same conditions may have been unfavourable to those animals that prey on the living polyps.

When I come to the next and last oscillations I will have to return to the coral islands; but I trust I have said enough to show that all the typical reefs and atolls are within what I have called the S. Pacific quarter of the sphere, and that they indicate an extensive depression of the land such as I have shown took place over the N. Atlantic quarter. I also trust that the facts and reasonings which I have adduced show that the formation of the atolls and barrier reefs of the Pacific do not necessarily represent a very long period of time.

This sub-section is already too long, but justice to my argument requires me to refer to some important evidence afforded by the distribution of birds. In his highly interesting work, the "Malay Archipelago," Mr Wallace has a chapter on the natural history of the Timor Group of Islands. In the three islands there are 188 species of birds, and with the exception of two or three species, which appear to have been derived from the Moluccas, all the birds can be traced either directly or by close allies to Java on the one side, or to Australia on the other, although no less than 82 species are found nowhere out of the small group of islands. There is not, however, *a single genus peculiar to this group*, or even one that is largely represented in it by peculiar species; and this is a fact which indicates that the fauna is strictly derivative, and that its origin does not go back beyond one of the most recent geological epochs.

As illustrating the foreign character of the birds, Mr Wallace makes the following summary:

Javan birds	36	Australian birds ...	13
Closely allied species ...	11	Closely allied species ...	35
Derived from Java	47	Derived from Australia	48

"The fact," continues Mr Wallace, "that the Australian species in these islands are mostly changed, while the Javan species have almost all remained unchanged, would therefore indicate that the district was first peopled from Australia. But for this to have been the case the physical conditions must have been very different from what they are now. Nearly three hundred miles of open sea now separate Australia from Timor, which island is connected with Java by a chain of broken land divided by straits which are nowhere more than twenty miles wide." As many characteristic groups of Australian birds are absent, and not a single Australian mammal has entered Timor, Mr Wallace does not think that the group of islands was ever actually united to Australia, but holds it as probable that the dry land reached to the edge of a submarine bank, which at one point approaches to within twenty miles of Timor. The bearings of this is obvious. At the beginning of the era the sub-marine bank referred to was, on my view, dry land; and this would enable the birds of Australia to reach Timor. Further, the same displacement of the equator that raised the continent of Australia depressed the opposite coasts of Asia. This again not only greatly widened the Malacca Straits, but it increased the distance from island to island; and thereby reduced the chances of birds reaching Timor from the north. Thus Mr Wallace's view with respect to the peopling of the Timor group of islands with birds from Australia leads exactly to the same conclusion as the glaciers of New Zealand and as the formation of the atolls and barrier reefs of the Coral Sea.

d. To convert the great submarine bank round the north coast of Australia into dry land required, as we have just seen, an elevation of 600 feet, and this elevation agrees very well with the amount of the depression on the opposite side of the equator. I have already had occasion to cite from a paper on the Stone Implements of the eastern coast of Southern India by R. B. Foot, Esq.; and it will be remembered:—"That the low-level gravels and conglomerates of the coast laterite and the coarse boulder gravels of the middle grounds, including the great accumulations of quartzite-shingle, were deposited or re-arranged into their present form during one and the same period, and are marine deposits; and that during the latter part of this period an elevating movement was in progress by which the land was raised between 500 and 600 feet."

The great accumulations of quartzite-shingle was doubtless true drift, for it extends over such a large area, filling up, as the drift everywhere does, the old river valleys, that it cannot be referred either to river-action or the tides and breakers. Mr Foot states that the gravels are marine deposits. If so, they could not have been deposited under ordinary marine conditions, and the upper part of them could have been but a short time under water, because they have not, like the shore margin, been converted into laterite by the precipitation of iron. But if the deposit belongs to the drift-zone, then in that case the "elevating movement" was simply the reciprocal of the Pacific depression; that is, during the depression when the atolls and barrier-reefs were being built up and receiving their general form, there was between the same meridians on the north side of the equator a corresponding elevation which round the Bay of Bengal was from 500 to 600 feet.

The Coast of China is everywhere so thickly covered with the modern alluvium deposited by the rivers that I have not seen any attempt to classify or unravel the Post-tertiary formations; however, Mr W. P. Blake has obtained evidence that the Island of Yesso has been elevated within the epoch of existing shells: "The next stratified formation of interest," says Mr Blake, "is marine Tertiary or Post-tertiary, which *rests unconformably* upon the older stratified beds, and is highly charged in some places with well preserved fossils, scarcely distinguishable from the mollusca now existing upon the coasts. In these deposits, and in late terrace-like formations, there is abundant evidence of the comparatively recent uplift of the whole island, and the same evidence is found in the island of Niphon." (Geol. Magaz. 1874.)

Going further east still, Mr Dawson of the Geological Survey of Canada, has shown that Vancouver Island has been elevated as well as the coast of the mainland of British Columbia. It will be remembered that there was also a great elevation in Siberia, where, according to Count Von Helmerson, as cited by Mr Gwyn Jeffreys, marine shells of living species occur nearly 500 miles south of the Icy Sea.

In both Patagonia and N.W. America there are terraces that carry the evidence of shore-action to much higher elevations than is done by the evidence afforded by sea shells. In the former country Dr Darwin observed them 1200 feet above the present high-water mark, and in the latter Mr Dawson traced the terraces from "a height of

5270 feet down to the present sea-level." Now, I cannot help thinking that sticklers for what physicists teach us as the fixedness of the axis have in these high terraces a hard nut to crack. It will not do to refer us to local depressions and upheavals, for we know from other evidence that the Andes and Rocky Mountains were upheaved at the close of the secondary era. Moreover, it is not to be overlooked that these high terraces, *without shells*, occupy the same geological zone at the opposite extremities of the continent; but, although on the same continent, they are not in the reciprocal quarters of the sphere, and for this reason we ought, in a cosmical change of level, to expect the sea to rise or fall, as the case may be, simultaneously in both regions; and the problem physicists have to solve is to account for the sea standing at such great elevations at points so remote from each other. On my view, however, the question admits of a very simple answer. When the great diluvial oscillation occured, the axis of rotation shifted in obedience to the mean momentum for the time being; but once shifted, the centrifugal force tended to keep the ocean in its new position. Besides, at this juncture the mean momentum contained other unstable factors:—(1) There were the concurrent engulfments and upheavals, accompanied by a general unquietness in the crust of the earth; (2) the land emerged with all its natural basins full to overflowing, and at the same time with all the old channels of rivers and streams choked up with drift; and (3) the effects on the momentum produced both by the emergence and submergence of large land areas.

It is of course possible that some of these disturbing causes acted as counterpoises to each other; still, considering the unequal distribution of land and water, the probability is that each must have contributed its share of force in preventing the return of the axis to a state of stable equilibrium. Now it was, as appears to me, during this transitionary state that the higher terraces in Patagonia and N.W. America respectfully were formed. My premises take for granted that strong currents and tempests of unexampled severity prevailed; and hence, a very short halt in the emergence of a mountain side would be sufficient to allow the breakers to cut an indentation into the surface of the newly deposited drift. Neither must it be forgotten that the terraces in question are continuous down to the existing shore line, nor that there are, in both regions, marine shells in the lower terraces, but none in the upper ones.

SEC. XX.—THE LAST COSMICAL CHANGE OF LEVEL.

NORTH ATLANTIC.	SOUTH ATLANTIC.	SOUTH PACIFIC.	NORTH PACIFIC.
a. Gradual Elevation.	*b.* Gradual Depression.	*c.* Gradual Elevation.	*d.* Gradual Depression.

a, Facts have already been given which go to prove that during the great Post-tertiary oscillation the sea rose above its present level round the whole of the Atlantic borders of both Europe and N. America and also round the shores of the Mediterranean and Red Seas; and hence I need not do more here than point out that the subsequent upheaval was the last change of level over this quarter of the sphere. This is made quite clear by the system of terraces in the superficial alluvium on the east coast of N. America, as well as by the raised beaches round the shores of Europe. Moreover we have already seen evidence that the elevation took place since Europe was inhabited; and the plate of lead found in a canoe embedded in the alluvium of the Clyde proves that the shores of Scotland were frequented by a people possessed of the knowledge of metals. I have already made a citation from Philo, but it will be advisable to give the passage at greater length. Speaking of the retreat of the sea the philosopher proceeds: "The sea, as they affirm, is already somewhat diminished, and for proof of this fact we can appeal to the most celebrated islands, Rhodes and Delos, for these were in ancient times invisible, being overwhelmed by and sunk under the sea, *but by lapse of time*, as the sea gradually diminished, they, by slow degrees, rose above it, and came into sight, *as the histories* which are written concerning them record. And they used to call Delos Anaphe, confirming the account here given by both names, since when it appeared above the waters it became evident, having been formerly invisible and unseen."

"And in addition to these arguments they (the historians) adduce the facts that many great and deep bays and gulfs of vast seas have been dried up, and have become land, and have so turned out no insignificant addition to the adjacent country when sown and planted, and on that soil there is still left plenty of proof of such spots having formerly been sea, in the pebbles and shells and other things which are commonly washed up on the sea shore being found in them; on which account Pinder says respecting Delos:

> "Hail, island raised by God,
> Chosen abode
> Of fair Latona's son, with golden hair;
> Hail, ocean's youngest child,
> The last immovable domain
> That o'er his bosom smiled.
> Upraised from beneath the billowy main,
> Mortals may call you Delos, but the choir
> That dwells upon Olympus' height,
> Their chosen bards inspire
> To praise thee as earth's holiest light."

But the elevation that gave islands and dry land to Greece caused trouble in Egypt:

"The priests told me," says Herodotus, "as a proof of what they related concerning this country, that in the reign of Moeris, when the Nile rose to at least eight cubits, it irrigated all Egypt below Memphis; and yet Moeris had not been nine hundred years dead when I received this information. But now, unless the river rises sixteen cubits, or fifteen at least, it does not overflow the country."

It will be observed that the Egyptian priests restricted their complaint to the region between Memphis and the sea, just that part of Egypt where the rise of the land from the deposition of Nile mud is the least; and hence the failure of the river to irrigate the country could not have been due to this cause. Neither can it be maintained that the alarm may have been owing to a succession of seasons with a low Nile; for the allegation is not that the river had fallen, but that the same amount of rise had ceased to flood the delta. Manifestly such an elevation as I am advocating must, in the case of the Nile, have produced a two-fold effect: first, it would reduce the total fall, and this would give the river a tendency to overflow its banks; and secondly, the rise of the coast line would increase the fall at its mouth. This increased fall would not only add to the force and velocity of the current, but it would also scour out and deepen the channels of the river. This deepening process would, as the land rose, gradually creep up from the sea towards Memphis; and to the Egyptians this fall in the height of the river would appear as the ordinary rise due to the deposition of sediment. The above appears to be the only natural explanation of the statements made to Herodotus, and it is highly interesting, inasmuch as it goes to fix the last oscillation at from 300 to 400 B. C.

b. As my paper is already much too long, it is perhaps fortunate that as regards the reciprocal depression in the S. Atlantic, I must rely more on the quality than the extent of my proof. In his geological observations on S. America Dr Darwin has given a section of the coast and sea-bottom round the Island of St. Helena. In front of the cliff the sea-bottom is, it is said, "uneven and rocky only to the depth of five or six fathoms; outside this line to a depth of about 100 fathoms, the bottom is smooth, gently inclined, and formed of mud and sand. * * * * * * At greater depths than the five or six fathoms, it seems impossible, under existing circumstances, that the sea can both have worn away hard rock in parts, to a thickness of at least 150 feet, and have deposited a smooth bed of fine sediment. The author then proceeds not only to suggest that a gradual depression of the land would remove every difficulty, but to extend the conclusion to the high cliff-bound, horizontally-stratified shores of Patagonia." But the author furnishes us with other facts which appear to place the depression of Patagonia beyond dispute.

From two continuous lines of soundings carefully taken between Santa Cruz and the Falkland Islands it was found that the pebbles at the bottom of the sea *quickly and regularly decrease* in size with the increasing depth and distance from the shore, whereas in the gravel on the sloping plains, no such decrease in size was perceptible. The following table gives the average results of many soundings off the Santa Cruz:—

Under two miles from the shore many of the pebbles were of large size, mingled with some small ones.

DISTANCE FROM THE SHORE.	DEPTH IN FATHOMS	SIZE OF PEBBLES.
3 to 4 miles	11 to 12	As large as walnuts, mingled with smaller ones.
6 to 7 miles	17 to 19	As large as hazel nuts.
10 to 11 miles	23 to 25	From three to four-tenths of an inch in diameter.
12 miles	30 to 40	Two-tenths of an inch.
22 to 150 miles	45 to 65	One-tenth of an inch to the finest sand.

* * * * * * "At my request Mr Kent collected for me a bag of pebbles from the beach of White Rock harbour, in the northern part of the sound, between the two Falkland Islands. Out of these well-rounded pebbles, varying in size from a walnut to a hen's egg, with some larger, thirty-eight evidently belonged to the rocks of these islands; twenty-six were similar to the pebbles of porphyry *found on the Patagonian plains* which rocks do not exist *in situ* in the Falklands; one pebble belonged to the peculiar yellow siliceous porphyry; thirty were of doubtful origin."

Thus out of a miscellaneous assemblage, twenty-five per cent. of the pebbles belonged to Patagonia, and manifestly this points to the conclusion that a continuous shore-line at one time united the islands to the continent. And this is not all. On their first discovery the Falkland Islands were inhabited by the Patagonian wolf-like fox, the *Canis Magellanicus.* This animal could not have crossed 280 miles of sea; neither could it have been introduced by man, because the appearance of the individuals inhabiting the islands had varied so far from those on the continent that they were for a time looked upon as a different species. In these circumstances I feel justified in claiming it as evidence of a former dry land connection. The 100 fathom line of soundings connects the Falkland Islands with Patagonia; and it will be remembered that in Scandinavia the sea rose 600 feet above its present level; but the former region is nearer the meridian line of maximum effect (30° W.) than the latter; and hence the fall of the sea below the present level at the Falkland Islands would be somewhat greater than the reciprocal rise on the coasts of Scandinavia.

The deep cliffs cut into the shore line of Patagonia prove that the high water mark, as in the N. Atlantic, has been long stationary. And the gravel brought up by the dredge proves the same thing, and proves also that the stationary period was preceded by a gradual depression of the land.

The eastern coast of Patagonia is tempestuous and the sea shallow, and hence the great rollers must act on the bottom at a considerable distance from land. On a sinking shore the breakers excavate the superficial beds and re-deposit the debris between the high-water line and the line where the breakers cease to act at low water. Every wave that strikes the shore has a back-draught, and this back-draught moves any gravel in its course into deeper water. But whilst the effective moving force of a current directed against every particular pebble is in proportion to the square root of the diameter, the *vis inertiae* on the other hand is as the cube root: and for this reason a current with a decreasing velocity assorts gravel in accordance with the size of the pebbles. And in consequence of this law the pebbles within two miles of the Patagonian shore are large; from eleven to twelve fathoms, as large as walnuts; at seventeen fathoms, as large as hazel nuts; at twenty-three fathoms, from three to four-tenths of an inch in diameter; at thirty fathoms, two-tenths; and at forty-five fathoms, one-tenth of an inch *to the finest sand.* This last quality of the bottom is shown at 22 miles from the shore, but as we have no soundings between 12 and 22 miles, there is nothing to show that it does not begin at 13 miles, and it is worthy of notice that the bottom falls suddenly ten fathoms at 12 miles. It therefore appears that at a depth of about 250 feet the action, or rather re-action, of the waves and ground-swell on the bottom is sufficient to move only pebbles of *one-tenth of an inch* in diameter, and as this size continues from about 13 miles from the shore to 150 miles (mid-channel), it affords evidence that the existing stability of the coast line was preceded by a gradual depression. To see this we have only to bear in mind that the influence exercised by the waves on the sea-bottom decreases from the shore till it dies out altogether in deep water; and, further, that any action of the waves along the line where the one set of conditions ends and the other begins must be feeble and intermittent. In the case before us this feeble and intermittent action is proved by a sea-bottom of pebbles one-tenth of an inch in diameter *to the finest sand*, and as the same condition of the bottom extends 120 miles seawards, the fact gives us reason to believe that the depression was gradual. Any one who feels inclined to dispute this conclusion ought to remember that pebbles, however small, do not swim. The agency of currents is equally inadmissible, because the minute pebbles in mid-channel are identical in their mineral characteristics with the boulders and gravel covering the plains of Patagonia. Darwin "found them fusible like the porphyries of the great shingle bed." He "could even distinguish particles of the gallstone-yellow porphyry," "It was interesting," he adds, "to notice how gradually the particles of white quartz increased as we approached the Falkland Islands which are thus constituted."

If we keep the above facts and circumstances in view, we cannot, as appears to me, well avoid the conclusion, that since the deposition of the Pampas clays, Patagonia has undergone a gradual elevation sufficient in amount to unite the Falklands with the mainland; and that this was followed by a depression of at least 100 fathoms.

Before leaving these changes of level that have occurred in Patagonia since the beginning of the post-tertiary era it will perhaps be well to recall the reciprocal changes in the N. Atlantic: (1) When the era began the sea in the S. Atlantic stood so high as to cover the greater part of Patagonia; and in the N. Atlantic so low that there was dry-land connection between the British Isles and the continent of Europe. (2) At the close of the oscillation that followed, the sea on the one hand stood so low in the South that the Falklands became united to Patagonia; and on the other so high in the North that Scandinavia was submerged to a depth of 600 feet. And (3) during the last oscillation the sea in the S. Atlantic rose till the Falkland Islands were separated from Patagonia by the present channel, 280 miles broad, and at least 400 feet deep; whilst the reciprocal fall in the North gave large areas of dry land to both Europe and to the east coast of N. America.

c. In the Pacific quarter of the sphere the proofs of an elevation are abundant, but, unfortunately for my case, Dr Darwin's view is that the height and form of the coral atolls and barrier-reefs show that the change of level that produced them was one of depression, and that the instances of upheaval were only local, and as a rule restricted to volcanic groups of islands; and owing to the pleasing and effective manner in which the distinguished naturalist places all his views before his readers, his conclusions have been very generally accepted. To have such a high authority against me is of course discouraging; still I must take the liberty of pointing out that islands formed of lava and older rocks are more reliable finger-posts than coral atolls, subject as they are to so many varying conditions.

In section XIX. I have suggested that during the Post-tertiary depression the circumstances in the Pacific were in an unusual degree favourable to the growth of coral; but judging from the sedimentary beds that were formed, the oscillation was not a very slow one, and hence, it can readily be conceived how the walls of living coral did not rise at a quicker rate than the sea overhead. Manifestly the agencies that bring on the decay of coral structures, such as exposure to the air, excessive rains, and the operations of boring animals, act mainly along the low water level. Hence structures of the kind growing up vertically in a submerged condition and hardly keeping pace with the deepening of the sea, could not yield much detritus; and for this reason we may assume that both the outside and inside of the reefs were originally very perpendicular. When, however, the oscillation changed its direction one of the earliest effects would be to bring the submerged atolls and reefs to the surface; and once at the surface exposure to the air would destroy the polyps, and this again would convert the living into rotten coral rock. But trees of dead coral, however strong the framework, cannot withstand the surf, and the polyps cannot live more than six inches above the low water line; and hence during a gradual elevation of the land the structure would become more and more exposed to the action of the breakers. This would remove the dead coral rock as fast as it rose to the surface, and the injurious effects of the detritus thus set free would prevent the growth of those great breast-works of living coral to which the reefs are indebted for their outward extension as well as for those portions now above the level of the sea. The remark about the detritus must however be understood only in a relative sense; because although the sediment washed over the edge of the reef doubtless interfered with the healthy development of coral life, still, sufficient vitality must have remained to increase the reef seaward. It is also to be remembered that the atoll or reef was meanwhile being gradually truncated by the degrading action of the breakers, and this of itself would add to its width. Neither must it be forgotten that after a great "roller" makes its final plunge the broken water rushes onwards till its force is exhausted by friction and gravity, and hence in studying the effects of the waves, the breadth of the reef becomes an important consideration. So long as a reef remained narrow the waves probably made an uninterrupted passage over it, and at the same time planed it down probably below the sea level. But with the widening of the reef there would ensue a struggle between the water action, on the one side, and the tendency of pieces of the dead coral rock to remain *in situ*, on the other. In the end, patches of coral would be formed, and when such masses once secured a footing they would gradually receive fresh accessions from the wash of the reef. That this was the *modus operandi* appears to be proved by the fact that in many instances the detritus and blocks of coral constituting the upraised portions of the reefs are arranged in the form of concentric beaches, thereby affording evidence of the successive steps by which the sea retired from the inner ring. This is markedly the case in Christmas Island, where, according to Captain Cook, such ridges are to be seen a mile from the present beach; and it can hardly be necessary to observe that this is altogether inconsistent with a depression; because, according to Dr Darwin's view, the inner circles of the atoll have been built with material washed from the outside platform. For this reason wherever there is evidence of a succession in the deposition of the detritus, the uppermost layers on the seaward side ought always to overlap those on the lagoon side. But so far as I can gather, this is not the case. As is well known, the waves have wrenched large masses from the outer edge of the reef and thrown them on to the platform above, where they are constantly washed by the waves. Now a large boulder of coral lying in such a position ought, during a depression, to become gradually embedded in the reef; but instead of this being the case, Dana tells his readers, that below some of these masses the platform was at times six inches higher than on either side of the mass, owing to the protection from wear given to the surface beneath it. Now it can hardly be necessary to remark that such cases instead of proving the building-up of reefs during a depression, prove the wearing away of reefs previously built up, and in the barrier-reef of Australia we have evidence to the same effect. Along the inside of this great reef islands of triturated coral have been formed, and these are at heights that cannot be explained by any action of the breakers. For instance, Raine Island forms part of the reef, and must, we are told, have been elevated at least twenty-five feet. "It consists of a hard compact brecciated coral conglomerate," and is said to be representative of a class of other islands on the reef. Dana, in his "Corals and Coral Islands," cites Jukes as follows:

"I once landed close to the edge of the Australian barrier on the south side of the Blackwood channel in Lat. 11° 44′ S., on a *continuous mass of Porites which was at least twenty feet across*, and it seemed to pass downwards into the mass of the reef below water without any disconnection. It was *worn into pinnacles above*, so that two or three of us could stand in the different hollows without seeing each other; and it was one of a line of such masses that attracted our attention for a distance of three miles." With such facts before us how can we come to any other conclusion than that the reef has been elevated, and along with it the whole east coast of Australia. And to realize the importance of the evidence we have only to call to remembrance that the reef skirts the coast for a distance of 1200 miles. The idea that most of us have of a coral island is that of a flat, narrow circle of land, adorned with cocoa-nut trees, and rising just a shade above the level of an unfathomable ocean, which, by one or two channels, ebbs and flows into the enclosed lagoon. Now, the following facts given in a tabular form by Dana, will show that this picture is only partially true:

GROUP.	ISLAND.	ELEVATION IN FEET.
Paumotu Archipelago	Honden	2 or 3
,,	Clermont Tonnerre	2 or 3
,,	Nairsa or Dean's	6
,,	Elizabeth	80
,,	Metia or Aurora	250
,,	Ducie's Island	1 or 2
Harvey and Rurutu Groups	Atiu	12
,,	Mauke	Somewhat elevated
,,	Mangaia	300
,,	Rurutu	150
Tongan	Eua	300
,,	Tongatabu	50 to 60
,,	Namuka and the Hapaii	25
,,	Vavau	100
Savage Island		100
North of Samoa	Swains	2 or 3
,,	Fakaafo, or Bowditch	3
,,	Oatafu or Duke of York's	2 or 3
Scattered Equatorial Islands	Washington	2 or 3
,,	Jarvis's	8 or 10
,,	Malden's	25 or 30
,,	Penrhyn's	35
,,	Enderbury's	2 or 3
,,	Newmarket	6 or 8
Feegee Islands	Viti Levu and Vanua Levu, Ovalau	5 or 6
North of Feegee	Ellice	5 or 6
M'Askill Islands	Three in number	60 to 100

The great irregularity in the elevation of the above islands forms, of course, a great contrast to the prevailing uniformity of multitudes of atolls at present at the level of the sea; but this is nothing more than what might have been expected. It must be remembered that the sudden upheaval in the Pacific of, say, 2000 or 3000 feet on the meridian of the barrier reef necessarily brought many submerged islands and volcanic cones and craters to the surface, and others, if not to the surface, at least within the zone of depth at which the polyps build their structures. Moreover, the convulsions of the times and the removal of the vertical pressure must have greatly added to the extent and intensity of the volcanic action. To these facts and circumstances add the "drowned reefs" and atolls which must have been formed during the great tertiary depression, and we will have a sufficient number of foundations for all the superstructures which now give its peculiar character to the Coralline Sea. At the present day with a stationary level, many reefs are fathoms below the low-water mark; and hence we may assume that reefs of this class would not, during the depression that followed the Deluge, keep pace with the rise of sea level. And if they did not, then they must have sunk below the proper habitat for reef-building corals — in other words, they would become what is known as "drowned reefs."

When in these circumstances, the oscillation changed its direction, the elevation that ensued produced, as already observed, a three-fold effect: (1) The breakers by planing off the dead coral rock truncated the reefs: (2) As the elevation proceeded it brought other reefs to the surface, there to undergo the same levelling process: And (3) many of the "drowned reefs," although not actually at the surface, must have been brought sufficiently near it to be within the zone of coral life; and all such would in the end rise to be ranked with the low islands of the present day. Thus the effect of a great depression followed by an elevation must have been to reduce numerous reefs and atolls to a common level. However, as regards coral structures built upon a nucleus of more enduring material, and which, at the time of the change of level, had not only reached the surface, but had become consolidated into hard rock: as regards them, the probability is clearly in favour of numbers of them having outlived the struggle with the breakers, and in time becoming elevated islands. It will be observed of the elevated coral islands that almost every height between 300 feet and zero is represented, and to avoid error we must not forget that the great majority of the alleged low atolls have been classed, as such, on purely negative evidence.

"When the tide is three feet," says Dana, "beach accumulations of large masses seldom exceed eight feet above high tide, and the finer fragments and sand may raise the deposit to ten feet; but with a tide of six feet twice this height may be attained. With the wind and waves combined or on prominent points where these agents may act from opposite directions, such accumulations may be fifteen to twenty feet in height and occasionally thirty or forty feet."

This definition of low islands, it will be observed, is pretty comprehensive, and when we take it into consideration along with the fact that comparatively few of the atolls have been examined by persons competent to undertake the duty, and fewer still have been examined exhaustively, it will be seen how great is the risk that proofs of elevation have been overlooked; and this suspicion will be increased when it is remembered that many of the islands have been classed on a mere inspection from the deck of a passing vessel. The proofs of elevation range, as we have seen, from zero to a height of 300 feet; and it is interesting to find that the depth of water in the respective lagoons varies in a similar manner, as there is every variety of depth, from instances of nothing more than dry hollows, indicating the former sites of lagoons, to the average maximum of thirty to thirty-five fathoms. Proof of an elevation may also be drawn from the circumstance that the beaches round some of *the high non-coral islands without reefs* are partly made up of coral sand. Speaking of Santa Christina, one of the Marquesas group, Mr T. D. Bennet writes: "The coast is rocky, abrupt, and surf-beaten. No coral reef encircles and protects its shores, nor those of any other island of this group. Nevertheless the detritus of coral is abundant on the beaches round the island." Does not this appear to prove that the island was once fringed, and that the breakers during an upheaval removed the coral rock leaving the remains as coral detritus along the shores?

According to my view, the elevation in going south increased with the latitude; and from a maximum of about 150° E. long. it decreased on each side to about 60° E. and 120° W. respectively; and hence we ought to expect to find the greater number of elevated islands near the meridian line of 150° E. in one direction and along the southern margin of the Coralline Sea in the other. And this appears to be the case; for the admittedly high islands are mostly included either in such groups as the Paumotu, Harvey, Friendly, New Hebrides, New Caledonia, and Loyalty, all skirting the southern margin of the coral area, or in groups located near to the line of maximum effect, such as New Guinea, New Ireland, Ceram, Timor, &c. I will now add a few proofs of elevation.

Speaking of a visit to Kaiva Island, one of the Batchian group, Mr Wallace states that the coralline rock *rises to an elevation of 200 feet;* and the same author remarks on the Island of Goram: "About a hundred yards from the beach rises a wall of coral rock, ten or twenty feet high, above which is an undulating surface of rugged coral, which slopes downwards towards the interior, and then, after a slight ascent, is bounded by a second wall of coral. Similar walls occur higher up, and coral is found on the highest part of the island." The Ké Islands, again, we are told, consist entirely of coralline limestone, and as Mr Wallace "endeavoured to ascend the hills," it is clear that the elevation has been considerable.

According to the Rev. W. B. Clark, Lafu, one of the Loyalty group, is of coral formation, and there is proof that it has been elevated 250 feet. "In the case of Lafu the elevation does not appear to be connected with visible volcanic force, but it is highly probable that it is merely the result of the elevation of New Caledonia * * * * which is everywhere surrounded by fringing and barrier reefs, of which the Loyalty and Brittania groups *are elevated portions.*"

In the abstract from Dana I have given the elevation of Metia Island, one of the western Paumotus; but this island has one feature which requires to be noticed. It is small and about "two hundred feet in height; and it consists throughout of coral limestone. * * * In the face of the cliff *there are two horizontal lines, along which cavities or caverns are most frequent.*"

"Wolchonsky Island is," says Captain Wilkes, "of an oblong shape, ten miles in circumference; its north end is high. The island San Pablo is said to be higher than Teku and Archangel, and to have several cocoa-nut groves. Margaret is a small round lagoon island, two miles in circumference, *high and well wooded* in the north side."

The same navigator tells us that M'Kean's Island is composed of coral sand and blocks, and rises twenty-five feet above the level of the sea. Dean's Island: The rim of land consisted for miles of an even wall of coral, apparently six or eight feet above high tide. This wall was broken into rude columns, or excavated with arches and caverns; in some places the sea had carried it away from fifty to one hundred rods, and then there followed again a line of columns, and walls, with occasional arches as before. Starbuck's Island is, it is stated, "an elevated lagoon island, but the amount of elevation is not stated." Ellice's Island: "On the windward side of the largest island of the atoll there is," says S. G. Whitnell, Esq., "a small lagoon (dry at low water), shut in from the sea by a wall, twenty feet high, consisting of large masses of coral," and he regards this as evidence of elevation. Moreover, on the reef, which is a narrow ledge, "compact masses of coral rock were observed *in situ* rising four feet above low water mark." Apamama is said to be elevated six or seven feet. "We were also told," says Dana, "that Kuria and Nonouti were similar in having the reef elevated, though to a less extent."

Jarvis's Island: "No lagoon but a basin-like depression over its interior, which *at bottom is seven or eight feet above the sea*, and in which the lagoon once existed."—[Dana.] Is from eighteen to twenty-eight feet in height.

Birnie's Island: "No lagoon. A sandy flat, about ten feet high, except near the north-north-east extremity, where it is about twelve feet."

Enderbury's Island: "Beach rises eight feet at an inclination of thirty to thirty-five degrees. * * * * After this first slope it is horizontal for eighty to two hundred feet, and *then there is a gradual rise of three to four feet.* The *lagoon very shallow.*"

Paumotu Archipelago: Otuhu Island. "No lagoon." Margaret Island, "a small shallow lagoon with no entrance." Teku: "A small lagoon with no entrance."—[Dana.] Elizabeth Island: "The cliffs round Elizabeth Island, in the same archipelago are eighty feet high, and are composed, according to Beechy, of homogeneous coral-rock. Mangaia, in the Harvey group, and Rurutu, appear to have once existed as encircled islands, and their barrier reefs are now in parts three hundred feet above the level of the sea."—[Darwin.]

Friendly Islands, Eoua: "Around the shores, as was seen by us from shipboard, there is an elevated layer of coral-reef rock, twenty feet thick, worn out into caverns, and with many spout holes. Between the southern shores and the highest part of the island, we observed three distinct terraces. Coral is said to occur at a height of three hundred feet." * * * * * *

"Namuka, and most of the Hapaii cluster are stated by Cook to have abrupt limestone shores, ten to twenty feet in height. Namuka has a lagoon or salt lake at centre, one-and-a-half miles broad; and there is a coral rock in one part twenty-five feet high. * * * * Vavau, the northern of the group, according to Williams, is a cluster of elevated islands of coral limestone, thirty to one hundred feet in height, having precipitous cliffs, with many excavations along the coast. * * * * * * Swain's is fifteen to eighteen above the sea, where highest, and the beach is ten to twelve feet high. It is a small island with a depression at the centre, but no lagoon. * * * * * * Malden's is described by Byron as not over forty feet in height."—[Dana.]

Savage Island: "A reef of coral" says Forster, who accompanied Cook, "surrounds the whole coast at a short distance; but having found an opening where the surf was not dangerous, we landed and climbed up one of the adjacent rocks, where we posted some of our sailors and marines. It consisted entirely of a sharp and craggy coral." A very important point to notice here is that the reef appears to be of the usual kind; whereas, according to the Rev. Mr Williams, the coral on the island rises to a height of one hundred feet.

Penrhyn's Island: "According to Captain Ringgold of the Wilkes Expedition, it has a height of fifty feet. * * * * * * Baker's Island: The greatest height is twenty-two feet, showing some evidence of elevation. * * * * * * Charlotte Island, one of the northernmost of the Gilbert Group, has the reef-rock in some parts raised bodily to a height of six or seven feet above low water level, evidencing this amount of elevation. Apamama is stated to be similarly elevated."—[Dana.]

"In the Navigator or Samoan Archipelago, Mr Couthouy found on Manua many large fragments of coral at a height of eighty feet. * * * * New Hebrides: On these islands, Mr G. Bennett informs me

[Darwin] that he found much coral at a great altitude, which he considered of recent origin. * * * * * * At New Ireland, which forms the northern point of the Salomon Archipelago, both Labillardière and Lesson have described large beds of an apparently very modern madreporitic rock, with the form of the corals little altered. The latter author states that this formation composes a newer line of coast modelled round an ancient one."—[Darwin.]

Tahiti: Dana thinks there is no conclusive evidence of elevation in this island, but as he adds the statement that "the shore plains are said to rest on coral which the mountain debris has covered," his opinion appears inexplicable; more especially when we have the testimony of Ellis to the effect that "in many places the sea has thrown up an embankment along the shore, considerably higher than the intervening space between the shore and the mountains; extensive swamps are thus formed."

Tongatabou is described as low and level, but with parts 50 or 60 feet high, the whole consisting of coral "which yet shows the cavities and irregularities worn into it by the action of the tides."

M'Askill Islands, three in number, are all of coral, and from 60 to 100 feet high. Fais Island "is fringed by reefs; it is 90 feet high, and consists entirely of madreporitic rock."

"The range of islands *south of Sumatra*, a part of the south coast of Java, and the islands east of it, the west and east ends of Timor, portions of all the Moluccas, the Ké and Aru Islands, Waijiou, and the whole south and east of Gilolo, consist in a great measure of upraised coral rock, exactly corresponding to that now forming in the adjacent sea. * * * * * * Returning now to the Malay Archipelago, we see that the whole of the seas connecting Java, Sumatra, and Borneo with Malacca and Siam, are under 50 fathoms deep, so that an elevation of only 300 feet would add this immense district to the Asiatic Continent. The 100 fathom line will also include the Philippine Islands, and the island of Bali, east of Java. From this we should naturally conclude that the subsidence breaking up the portions of Asia, had recently taken place. * * * * * * On examining the zoology of these countries this opinion is confirmed, for we find the most overwhelming evidence that these great islands must have once formed a part of the continent, and could only have been separated at a very recent geological epoch. The elephant and tapir of Sumatra and Borneo, the rhinoceros of Sumatra, and the allied species of Java, the wild cattle of Borneo, and the kind long supposed to be peculiar to Java, are now known all to inhabit some part or other of Southern Asia." [Mr Wallace, in the Geographical Journal Vol. XXXIII.]

It will hardly be necessary to observe that all this is in perfect harmony with my argument. The post-tertiary era began with the Equator some 10° or 15° farther north on the Meridian of 150 E. long. This almost, if not altogether, formed a dry land connection between New Guinea and Australia, whilst at the same time, it increased the distance between Sumatra and the Continent of Asia. But the Equator gradually travelled south, and this increased the distance as well as the depth of water between the islands of the Archipelago and Australia, and at the same time it united Sumatra to the Malay Peninsula. The oscillation, however, changed its direction, with the effect that all the dry land south of the Equator rose higher, and that to the north of it sunk more below the sea. And during this last change the formation of the existing Straits of Malacca cut off the dry land connection between the western division of the Archipelago and the Asiatic continent.

Mr Wallace also points out that the fauna connect New Britain and the Salomon Islands with New Guinea; and this is important. "M. Lesson states," says Darwin, "that near Port Dory, on the north coast of New Guinea the shores are flanked to the height of 150 feet by madreporitic strata of a modern date." He mentions similar formations at Waijion, Amboyna, Bouru, Ceram, Souda, and Timor; at this latter place, M. M. Quoy and Gaimard have likewise described the *primitive rocks* as coated to a considerable height with coral! All these islands are on the west side of New Guinea, but we find corresponding facts on the east and south sides; for there are proofs of elevation in the Admiralty Group, New Britain, the Salomon Islands, New Hebrides, the Louisiade Archipelago, and the Loyalty Group.

We have already seen that the Great Barrier reef has been elevated, and, as it skirts the coast for 1000 miles, we may safely assume that the whole east-coast of Australia has shared in the elevation. In the

Geological Journal Vol. XXV., Dr Rattray shows that not only the reef has been elevated, but that the peninsula of Cape York is to a large extent covered by a post-tertiary formation: and the same elevation extends south to Tasmania. In a paper which I read before the Tasmanian Royal Society a few weeks ago, says Mr S. H. Wintle, "I pointed out that until a very recent period in the Geological annals of this island, a great portion of what now constitutes the site of Hobart Town had been under water. This is proved by the extensive deposits of comminuted shells all of recent species, which are met with for miles indeed, along the banks of the Derwent. Some of these deposits are at an elevation of upwards of one hundred feet above high water mark, and from fifty to one hundred yards from the water's edge, plainly showing thereby that a very recent elevation of the land has taken place."

But the elevation was not restricted to Van Diemen's Land. In an account of an expedition from Adelaide to Mount Gambier in South Australia, by Governor Grey, Deputy Surveyor-General Burr notes the following facts: "We passed four wells on this day's journey, which had been dug by different overland parties at their camping ground [near Cape Bernonilli], and we dug a fifth at our own camp. Water was obtained at 5½ feet, in a bluish sand, which lay beneath a white sand. In the sand were numerous shells in a perfect state of preservation, and either belonged or were nearly allied to those species which are now found on the coast." * * * * * At Ross's Creek "a well was dug through sand, and tolerable water obtained at 5 feet. There were many recent shells in the sand, and a recent sandstone with shells in it; the shells were either identical with, or nearly allied to, existing species; at about four feet below the surface there was a thin bed of sea-weed between recent sandstone; the sea-weed had every appearance of that at present on the coast."

In the Geol. Journal, vol. 16, the Rev. J. G. Woods writes of the same district in S. Australia: "A ridge of coarse limestone follows the line of coast; and in this, as well as in the limestone some few miles further inland, fossils abound; but they are all of species at present inhabiting the coast. This is the result of upheaval which appears from observations to continue to this day." As regards Western Australia, Mr F. T. Gregory, in vol. 17 of the same journal, writes: "Between the west escarpment of the table-land or Darling Range and the sea, lies a low belt of country from 20 to 60 miles in width, the upper portion of which is evidently of very recent formation, and at no very distant period has been submerged under the ocean. From Flinders Bay, northward to the Moore river, this tract of country does not generally attain to a greater elevation than from 40 to 200 feet, only reaching the latter elevation where the action of the wind has driven the sand from the sea in ridges running parallel to the coast." From the evidence furnished by the barrier-reef I have inferred that the rise of the land was equable along the whole eastern border of Australia; and as regards Western Australia, Mr Gregory states: "On the surface of the older palæozoic strata the carboniferous rocks were deposited unconformably. They were again denuded, and cretaceous and tertiary rocks were deposited unconformably on both. Since then the country has been both elevated and depressed, more than once, perhaps, *but bodily and equably*, and without tilting."

New Zealand: Dr Hector, in a letter to the late Sir R. Murchison, refers in the following terms to the Post-tertiary depression in the province of Otago: "Distinct evidence of the Post-tertiary oscillation of the coast-line resulting in emergence is only to be found in the eastern, southern, and north-western parts of the provinces."

"Striking proofs of the elevation of the east coast are," says Dr F. Von Hochstetter "presented especially in the environs of Bank's Peninsula. This peninsula seems to have been an island till within a very recent age. It is connected with the mainland only by a very low neck, the numerous lagoons of which, and especially the great Lake Ellesmere on the S.W. side of the peninsula, may be considered remnants of the former sheet of water covering the intervening tract of land. Two miles inland, within a semi-circle extending from the mouth of the Waimakariri to Lake Waihora, lies a chain of old sand hills that once constituted the coast."

Captain Hutton, in a synopsis of the younger formations of New Zealand, gives the following localities for obtaining marine fossils of the Pleistocene period: North Island, Thanganui (upper series); Cape Kidnappers (north-west side), South Island; Montanau, Canterbury.

With so many facts and circumstances before us the question may well be put—To what conclusion do they all lead? In the S. Pacific quarter of the sphere, as already defined, there is not a non-coral island of any size that does not to this day bear proofs of upheaval. And this is not all. Numerous coral islands are themselves elevated, the extent ranging from zero to a height of 300 feet. Further, the lagoons of the atolls classed as low ones vary from mere dry hollows to a depth of from thirty to thirty-five fathoms, and there are instances of even 45, 48, 54, and 56½ fathoms; and to my mind such great inequalities in the soundings afford proof in support of a general elevation, almost as good as that furnished by the upheaved coral islands themselves. Neither will the reader have forgotten that the great Barrier-reef of Australia, with its numerous islands both on and inside the reef, and with a length of nearly 1200 miles, proves the same thing; nor how the New Caledonian reef, 400 miles in length, is, in the opinion of the Rev. W. B. Clarke, only a portion of a wider elevated area that includes the Loyalty and Britannia groups—Lafu in the former and Britannia Island in the latter being each 250 feet high. Or looking at the evidence from the different points of the compass, we find proofs of elevation from the Caroline Archipelago in the north to Van Dieman's Land and New Zealand in the south; or, if we begin at Sumatra in the west, proofs of elevation may be traced to the Low Archipelago in the east. In short, there appears to be proof of an elevation wherever the circumstances were favourable for preserving the evidence of the fact.

D As in the case of the S. Atlantic we may expect to find in the N. Pacific only scanty proofs of a depression of the land; and from the necessities of the case these proofs can only be of an indirect kind. I have already cited the conclusions arrived at by Mr R. Bruce Foot, that since the deposition of the gravels now constituting the laterite deposits round the Bay of Bengal, and which contain human remains, the land has been elevated from 500 to 600 feet. But it did not remain at this level. "The elevation was followed," says Mr Foot, "by a period of quiescence, during which the laterite deposits underwent very extensive denudation, particularly from the action of the rivers, which cut passages for themselves across the belt of recently raised land, and in great measure scoured out the laterite deposits from the old river valleys which had been scooped out of the gneissic rocks, probably in much more ancient times. To this period of quiescence succeeded a period of depression, but to what depth it proceeded we have as yet not sufficient evidence to show. Probably it was not very great."

On going east to the Straits of Malacca, we have proof of the same depression. "The general result, therefore, at which we arrive is," says Mr Wallace, "that the great islands of Java, Sumatra, and Borneo resemble in their natural productions the adjacent parts of the continent, almost as much as such widely separated districts could be expected to do even if they still formed a part of Asia; and this close resemblance, joined with the fact of the wide extent of sea which separates them being so uniformly and remarkably shallow * * * * * * * all lead us irresistibly to the conclusion that at a very recent geological epoch the continent of Asia extended far beyond its present limits in a south-easterly direction, including the islands of Java, Sumatra, and Borneo, and probably reaching as far as the present 100 fathom line of soundings." Now if we accept Mr Wallace's conclusion it is clear that a considerable depression was required to cut off the islands in question from the continent of Asia; and it is important to remember that the proof which the fauna of these island affords is not confined to a spot, but extends for 1000 miles in an east and west direction. It is true Mr Wallace attributes the depression to "the existence of the extensive range of volcanoes in Sumatra and Java, which have poured out vast quantities of subterranean matter, and have built up extensive plateaux and lofty mountain ranges, thus furnishing a *versa causa* for a parallel line of depression." But he himself appears to rebut the suggestion, for he tells us "It is interesting to observe among the islands themselves how a shallow sea always intimates a recent land-connection. The Aru Islands, Mysol and Waigiou, as well as Jobie, agree with New Guinea in their mammalia and birds much more closely than they do with the Moluccas, and we find that they are all united to New Guinea by a shallow sea. In fact, the 100 fathom line round New Guinea marks out accurately the range of the true Paradise birds." (Geog. Jour., vol. XXXIII.)

From Amoy in China to the Peninsula of Kamtschatka, there is a submarine bank, studded with a chain of islands, and the terrestrial fauna on all these islands appear to be almost identical. The island of Formosa is at the western end of this chain, and now ninety miles from the continent of Asia, and its fauna includes bears, monkeys, leopards, tigers, civets, weasels, squirrels, hares, muntjaks, antelopes, deer, and moles. The larger and more important islands constituting the Japanese Empire include in their list of wild animals wolves, foxes, deer, antelopes, buffaloes, the black and brown bears, badgers, monkeys, moles, squirrels, weasels, polecats, and otters. Between Japan and Kamtschatka there is a chain of smaller islands, known as the Kurile group, and they have bears, wolves, foxes, beavers, hares, polecats, sables, weasels, and moles. Doubtless these lists do not exhaust the fauna; still, I think they are sufficient to show that what is now a submarine bank extending from Formosa in the west to Kamtschatka in the east, that is about 3000 miles, was at a late geological date a continuous stretch of dry land; and that a subsequent rise of the sea submerged the greater part of it, leaving only the higher regions, as so many islands insulated not only from each other, but by wide seas from the continent of Asia.

Again, Behring's Straits afford us the same kind of evidence. The following list of animals is incomplete, still it will convey an idea of the terrestrial animals that are common to Asia and N. America:

Reindeer.
Gluttons.
Badgers.
Wolves.
Foxes.
Otters.
Polecats.
Weasels.
Polar Bears.
Lemmings.
Lynxes.
Moose.
Argali.
Polar Hares.
Moles.
Shrews.
Pikas.
Water Rats.
Bisons.
Wild cats.
Pine Martin.
Beavers.
Black Bears.
Brown Bears.
Voles.
Squirrels.

I freely admit that most of these animals could have crossed Behring's Straits on the ice; but that, by this means, they all did so is from every point of view improbable. For instance, what could have induced such animals as bisons, wild sheep, hares, moles, pikas, squirrels, wild cats, pine martins, and beavers to start on such a journey? The wild sheep or argali is now a native of the mountains of Siberia on the one hand and of the Rocky Mountains on the other, and it appears unlikely that it should not only have descended to the sea-coast but have travelled onwards for fifty miles over ice. As regards the pikas, Mr J. R. Lord, who observed their habits in the Cascade Mountains, says, "I soon observed they were busy at work, carrying in bits of dry grass, fir fronds, roots, and moss, and constructing a nest in the clefts between the stones, clearly for winter sleeping quarters." Add to this that the little animal feeds upon grass, and is, including head and body, only 6½ inches long, and that it hibernates in winter, and it will be seen how impossible it was for it to emigrate from the Old to the New World by taking advantage of a bridge of ice. Again, are we to suppose that beavers and squirrels forsook their winter houses and natural habitats to launch forth into what to them must have been a *terra incognita.* Or granting that some adventurers did start on an exploring expedition, were there not a thousand chances to one against their going straight across? It is to be remembered we are not speaking of herds of migratory animals whom experience has taught to change their latitude with the temperature of the seasons, but which, by the way, travel both going and returning by the same route. We are not speaking of such, but of creatures whose range is restricted by well-defined conditions. The case of the mole is very striking. It can swim well, but cannot live more than a few hours without food, and hence to it at least a journey from the one continent to the other was impossible; and yet the *talpa insularis* has been found in both Asia and California. Another circumstance in support of a former land connection is that although the numerous animals adverted to above as common to both sides of Behring's Strait, are of the same species, yet the individuals on the Asiatic side can be distinguished by some feature from those of America; and this clearly indicates that the migration has not been recent, and, if not recent, neither can the date have been very old in a geological sense. It will be remembered that the terraces of N.W. America range from a height of upwards of 5000 feet down to the present sea level. And according to Mr W. H. Dall (American Journal, vol. 45), the entire territory of the Yonkou River "is sprinkled with remains of *Pliocene* animals. * * * * * * The Kotto River, emptying itself into the Yonkou, is held in superstitious dread by the Indians on account of the immense number of fossil bones existing there. The Inglutàlic River, emptying into Norton Sound, has a somewhat similar reputation." When to the above we add the well-known collection of mammals found in the "diluvial clay" at Kobsebue Sound it will be seen that it is the extinct and not the living fauna that are associated with the surface deposits of Alaska Territory. "It still appears to me," says Mr George M. Dawson, writing of this region, "most probable, however, that this stage of the glacial period was closed by *a general submergence*, during which the deposit referred to as boulder clay was laid down in the interior plateau, and as the land again rose it assumed its present terraced character." Now, it seems to me that the Asiatic fauna could not have crossed to America during the terrace period, because the depression of the land which the terraces require must have greatly broadened the strait. Doubtless, I may be referred to floating ice—the ready transport ship in all such cases of difficulty; but unfortunately for the suggestion, there is a constant current flowing north through Behring's Straits, and this must have reduced to a minimum the chances of a floe passing from the one continent to the other. Moreover, during the terrace period, with a broader and deeper channel, it seems extremely doubtful whether ice could have formed in the strait at all. In describing some superficial deposits with marine shells in Vancouver Island, Mr Dawson informs us that "the somewhat irregularly terraced form of the deposit is probably due to action during emergence; and the general tendency of many facts is to show that a *slight sinking of the coast* is at present in progress or has lately occurred." Here the sinking is said to have been a recent occurrence; but the author proceeds to state a fact which shews that it must have taken place a long time ago. "Occasionally artificial sections at New Westminster, at the mouth of the Frazer River, on the opposite side of the Strait of Georgia, *show deposits quite similar* in general features to those seen near Victoria, but no molluscous remains have been found. *These beds pass under the modern flat and wide delta of the river, which extends many miles seaward.*"

Mr Dawson does not specify the nature of the facts from which he infers a sinking of the coast of Vancouver Island, but whatever may have been its amount, the fact that on the opposite side of the channel the delta of the Frazer extends many miles seaward suggests that the sea-level has been long stationary at the spot, because a sinking shore line ought to cause the delta to recede. New Westminster is, as I have somewhere seen stated, on one of the old terraces, and the circumstance that the surface out of which the terrace is formed there passes under the more modern delta, seems to show that the shore-line must have receded, but at a date remote enough to allow sufficient time for the subsequent extension of the delta seaward.

The depression that separated the Falklands from Patagonia was treated, it will be remembered, as the reciprocal of the elevation of 500 feet in the north of Scotland and of 600 feet in Scandinavia, but as Scotland is 30° to the east of the meridian, which I have taken as the line of maximum effect, the corresponding depression at the Kurile Islands would be greater, and sufficient to unite the chain of islands that have just passed under our review to the coast of China at the one end and to the Corea and Kamtschatka at the other. It would also cut off the dry land connection between the Old and New Worlds.

From the facts and reasonings given above, such changes seem actually to have taken place, or, at any rate, the amount of depression that I have suggested would remove all the difficulties regarding the distribution of the terrestrial mammals among the Asiatic Islands—difficulties that run along a chain from the Bay of Bengal to Behring's Straits.

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